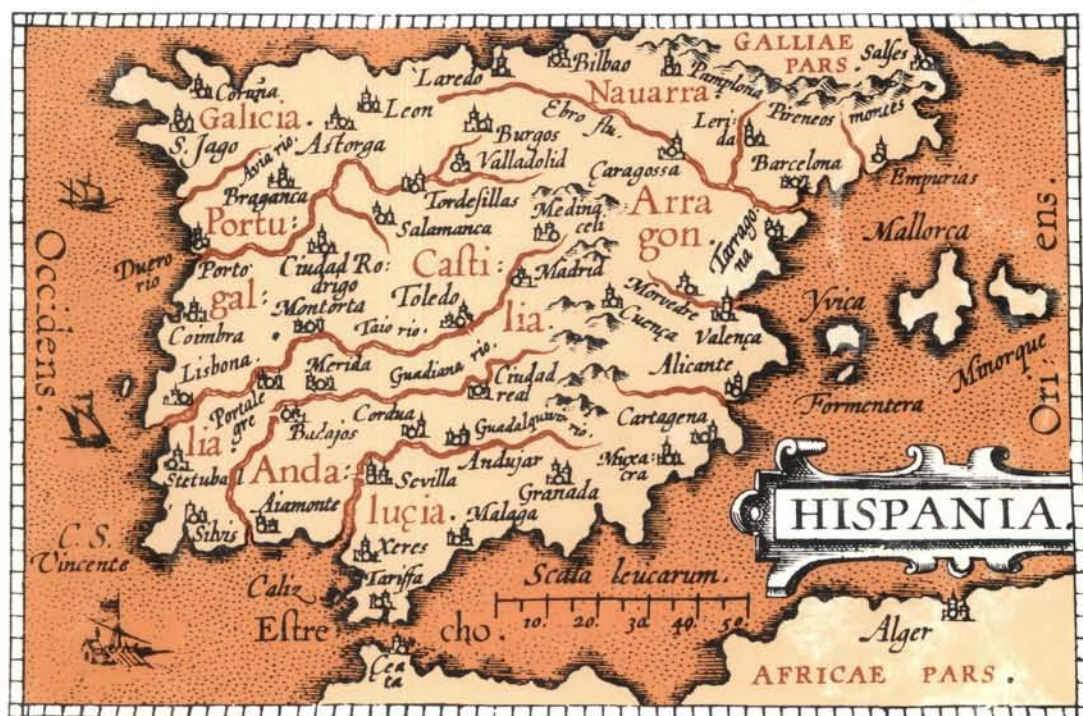
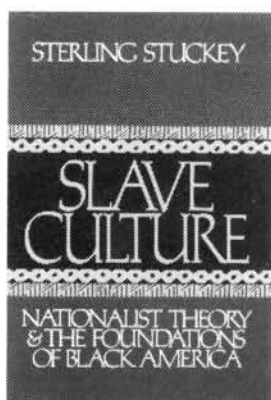


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# The American Historical Review

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## In This Issue

Among the larger historical enquiries encompassed by the forthcoming Columbus Quincentennial, **Carla Rahn Phillips** examines the economy of early modern Spain on its own terms. Surveying the enormous outpouring of scholarship on Spanish economy and society in the sixteenth and seventeenth centuries, Phillips provides a synthesis of current research little known among American scholars of early modern Europe. Three broad geographical regions of Spain distinct in climate and topography nevertheless shared certain demographic and economic patterns. Even though regional differences characterized the balance drawn between resources and population, the Spanish experience as a whole fit within the Malthusian world of limited resources and individual adaptation that characterized all of early modern Europe.

In order to identify and weigh the relative contributions of African and European settlers to the black societies of the New World, **Cheryll Ann Cody** focuses on the naming practices of slaves. Drawing from the surviving documents of the South Carolina Ball family's rice plantations, Cody is able to follow the transmission of slave names over the course of several generations, from 1720 to 1865. Reconstructing lineages from the names of 2,344 slaves born on the plantations, Cody demonstrates that slaves avoided the names of male members of the Ball family but not the names of plantation mistresses. African day-naming practices were steadily replaced by the preference for biblical names during these 145 years. In the selection of names, slaves emphasized kin ties that included both generational depth and breadth, especially after 1780. In contrast to their white masters, the Ball plantation slaves of the early nineteenth century drew from a wide pool of names introduced among the first two generations of Ball slaves. In so doing, the slaves maintained significant ties between the present and distant past. Nevertheless, as the antebellum period drew to a close, biblical naming practices revealed a new emphasis on male kin, necronymic naming, and a model of patrilineal society that resembled the practices of the slaves' owners.

**Mary Kupiec Cayton** studies the midwestern audiences attending lectures by Ralph Waldo Emerson. Emerson enthusiasts in Cincinnati and other midwestern cities heard his message selectively, in ways consonant with their own material circumstances. The economic class that helped popularize Emerson solidified its own notions of culture by selectively "misreading" Emerson. Exploring in a larger sense the boundary between a social history of a prominent intellectual and the history of an ideology Emerson fashioned, Cayton offers another way to assess the historical importance of prominent intellectual figures than the study of the intent or extent of their writings.

As we head toward 1992, we need to reexamine the current state of knowledge about the Columbian legacy in the Americas and in the new Atlantic world that Columbus helped create. **James Axtell** argues that American history textbooks are poorly equipped to convey the richness of that multi-cultural heritage and therefore unable to render intelligible the colonial origins of the United States. In examining sixteen popular textbooks, Axtell finds that the space devoted to the Age of Discovery is woefully inadequate. Factual errors abound; illustrations are poorly chosen; the portrayal of the Spanish empire is skewed by the "Black Legend" of Spanish conquest through cruelty. That bias gives the Spanish unwarrantedly greater attention than is accorded the French. He urges academic and publishing circles to reform American history texts.

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## Contributors

**James Axtell** is Kenan Professor of Humanities at the College of William and Mary. He received a Ph.D. from Cambridge University, where he studied Tudor-Stuart history. Most recently, he is the author of *The Invasion Within: The Contest of Cultures in Colonial North America* (1985), which won three awards, including the Albert B. Corey Prize from the American Historical Association-Canadian Historical Association. He is at work on the second volume of a proposed trilogy on the cultural interactions between the colonial French, English, and Indians. Oxford University Press is publishing a new collection of his essays in 1988.

**Mary Kupiec Cayton** is a visiting assistant professor of interdisciplinary studies at Miami University, Oxford. She received her Ph.D. in American Civilization from Brown University in 1981, having worked with John L. Thomas and Hyatt Waggoner, and spent a postdoctoral year at Boston University studying literary theory and New England Transcendentalism. She is currently completing a book-length manuscript on Emerson's early years and has begun work on a project that examines the process by which female writers negotiate a relationship with male-defined discourse communities. Her interest in Emerson's midwestern audiences grew out of her experience in writing for multiple audiences and a National Endowment for the Humanities Summer Seminar for College Teachers on "Democratic Culture in America, 1770–1870" at Indiana University.

**Cheryll Ann Cody** is an assistant professor of history at the University of Houston. She received her Ph.D. from the University of Minnesota and has just completed a book manuscript on the family lives and demography of the Ball family slaves. Her ongoing research includes a study of marriage and inheritance among the South Carolina Low Country planters and an analysis of the fate of the Good Hope plantation people in Arkansas and Texas after emancipation. Her previous essays have appeared in *Southern Studies* and the *William and Mary Quarterly*.

**Carla Rahn Phillips** is a professor of history at the University of Minnesota, where she has taught since 1972. She currently serves as general secretary of the Society for Spanish and Portuguese Historical Studies. Her research centers on the economy and society of Spain in the early modern period. In addition to articles on population, family structure, landownership, merchants, and trade, she has published two books: *Ciudad Real, 1500–1750: Growth, Crisis, and Readjustment in the Spanish Economy* (1979) and *Six Galleons for the King of Spain: Imperial Defense in the Early Seventeenth Century* (1986). She was recently awarded a Guggenheim fellowship to write a book on the Spanish wool trade, in collaboration with William D. Phillips, Jr. In recent years, her interests have turned increasingly to the problems of maritime exploration and trade, and to the global impact of the European voyages of discovery. In anticipation of the Columbian Quincentenary in 1992, she and William D. Phillips, Jr., will write a study of Columbus and his world for Cambridge University Press.



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## Time and Duration: A Model for the Economy of Early Modern Spain

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CARLA RAHN PHILLIPS

EVERY FIELD OF HISTORICAL STUDY HAS ITS INESCAPABLE QUESTION, one that looms ahead wherever scholars turn for inspiration. For those of us in Spanish history of the early modern period, that question concerns the decline of Spain. Why did Spain, having created a worldwide empire and achieved political hegemony in the sixteenth century, decline in the seventeenth century to second-rank status? (The equally important question of the rise of Spain, rooted in the medieval centuries, has yet to be adequately addressed.) By posing the question of decline in political terms, historians have usually found political answers, tracing the fortunes of the country through the characters and administrations of successive monarchs. The common interpretation holds that the rise of Spain characterized the sixteenth century, and its decline characterized the seventeenth, with the reigns of the Spanish Habsburgs neatly encompassing the whole. The new Bourbon dynasty in the eighteenth century presided over a revival of Spanish fortunes both at home and abroad. Implicit in many studies has been the assumption that decline could have been avoided by a change in governmental policies or human behavior. The economy of Spain has often been presented as a dependent variable, reacting to the government, rising and falling with the political importance of Spain and the competence of its leaders. The only economic variables that have acquired lives of their own are the flows of silver and trade to and from the Spanish empire.

There is considerable merit in attempting to integrate political and economic realities in the same analysis, as many recent studies of Spain demonstrate.<sup>1</sup> The

Earlier versions of this article were presented to the Society for Spanish and Portuguese Historical Studies and the Early Modern History Group at the University of Minnesota.

<sup>1</sup> The best examples are John C. Lynch, *Spain under the Habsburgs*, 2 vols. (Oxford, 1965, 1969; 2d edn. rev., 1981); John Huxtable Elliott, *Imperial Spain, 1469–1716* (New York, 1963); Antonio Domínguez Ortiz, *El Antiguo Régimen: Los Reyes Católicos y los Austrias*, 6th edn. (Madrid, 1979); Antonio Domínguez Ortiz, *La Sociedad española en el siglo XVII*, 2 vols. (Madrid, 1963, 1970), summarized as *Crisis y decadencia de la España de los Austrias* (Barcelona, 1969); Manuel Fernández Álvarez, *La Sociedad española del Renacimiento* (Salamanca, 1970); and Jaime Vicens Vives, *An Economic History of Spain*, 3d edn. rev., with the collaboration of Jorge Nadal Oller, trans. Frances M. López-Morillas (Princeton, N.J., 1969). Other Spanish work is cited in Valentín Vázquez de Prada, "La Historia económica en España desde 1940," *Conversaciones internacionales de historia*, III: *La Historiografía en occidente desde 1945* (Pamplona, 1985): 429–69. Recent shorter studies of varying worth include J. H. Elliott, "The Decline of Spain," *Past and Present*, 20 (1961): 52–75; J. H. Elliott, "Self-Perception and Decline in Early Seventeenth-Century Spain," *Past and Present*, 74 (February 1977): 41–61; Henry Kamen, "The Decline of Spain:

difficulty comes, however, as scholars begin to realize that political and economic realities had different patterns of ascent and decline. The rise of the Spanish economy began in the politically chaotic fifteenth century.<sup>2</sup> The reign of Charles I, which established Spain's political hegemony, included disturbing economic trends such as the increasing overextension of royal finances. And the reign of Philip II, indisputably the peak of Spanish political power, was witness to economic distress. Even more unexpected has been the discovery that the reigns of Philip IV and Charles II, which experienced unrelieved political decline, saw an economic revival. The new Bourbon dynasty fostered its continuation, but the revival had begun several decades before the Bourbons inherited the throne.

These "overlapping histories, developing simultaneously" complicate the search for a comprehensive synthesis.<sup>3</sup> Before we can understand the whole, we must understand its parts, and the most misunderstood part of Spanish history has been its economy. Given the paucity of detailed research on Spanish economic development, many syntheses have relied too heavily on striking quotations and decorative statistics. At best, such expedients can stretch thin evidence into a plausible and colorful narrative. At worst, they can distort and misrepresent that evidence. The concept of a single rise and decline may not, in fact, be a useful framework for the early modern economy.

An accumulation of published work now makes a tentative synthesis possible. Not long ago, scholars depended on a few classic works of Spanish demography, some of them dating from the early nineteenth century.<sup>4</sup> In the last several decades, a profusion of local demographic and economic studies has appeared in Spain, many of them produced for master's and doctor's degrees. Although many

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A Historical Myth?" *Past and Present*, 81 (November 1978): 24–50; and the persuasive critique of Kamen by Jonathan Israel, "Debate—The Decline of Spain: A Historical Myth," *Past and Present*, 91 (May 1981): 170–85; James Casey, "Spain: A Failed Transition," in *The European Crisis of the 1590s: Essays in Comparative History*, Peter Clark, ed. (London, 1985), 209–28. Kamen and Casey approach the topic negatively and betray a lack of understanding of the way in which the economy functioned.

<sup>2</sup> William D. Phillips, Jr., *Enrique IV and the Crisis of Fifteenth-Century Castile, 1425–1480* (Cambridge, Mass., 1978).

<sup>3</sup> Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, Siân Reynolds, trans., 2 vols. (New York, 1972), 2: 892.

<sup>4</sup> Pascual Madoz, "Población de España," *Anuario estadístico de España* (Madrid, 1859–60), xxv–xxxviii; Albert Girard, "Le Chiffre de la population de l'Espagne dans les temps modernes," *Revue d'histoire moderne*, 3 (November–December 1928): 420–36; (January–February 1929): 3–17; Albert Girard, "La Repartition de la population en Espagne dans les temps modernes," *Revue d'histoire économique* (1929): 247–362; Javier Ruiz Almansa, "Las Ideas y las estadísticas de población en España en el siglo XVI," *Revista internacional de sociología*, 5 (July–September 1947): 89–107; Javier Ruiz Almansa, "La Población de España en el siglo XVI," *Revista internacional de sociología* (1943): 115–36; Javier Ruiz Almansa, *La Población de Galicia: 1500–1945* (Madrid, 1948); José Ros Jimeno, et al., *Estudios demográficos*, 2 vols. (Madrid, 1945–47); and Agustín de Blas, *Origen, progresos y límites de la población y examen histórico-crítico de la de España . . .* (Madrid, 1833). The most remarkable work is José de Vargas y Ponce, *Estados de vitalidad y mortalidad de Guipúzcoa en el siglo XVIII (1805)*, Gonzalo Anes y Alvarez de Castrillón, ed. (Madrid, 1982). Including birth, marriage, and death figures for population centers in Guipúzcoa for much of the eighteenth century, it will prove invaluable for future demographic studies. The manuscript has been housed in the Real Academia de la Historia. Anes's careful edition and facsimile reproduction of the tables make it fully accessible for the first time. More modern syntheses include Jorge Nadal, *La Población española (siglos XVI a XX)* (Barcelona, 1966); Jorge Nadal and Emilio Giralt, *La Población catalane de 1553 à 1717: L'Immigration française et les autres facteurs de son développement* (Paris, 1960); and Domínguez Ortiz, *Sociedad española en el siglo XVII*.

of the studies are well researched and technically sophisticated, they often lack a sense of broader context.<sup>5</sup> It might be preferable to wait until more work has appeared, but, as Marc Bloch reminds us, "There are moments in the development of a subject when a synthesis, however premature it may appear, can contribute more than a host of analytical studies; in other words, there are times when for once the formulation of problems is more urgent than their solutions."<sup>6</sup>

This article surveys the available evidence on the early modern Spanish economy, concentrating on the sixteenth and seventeenth centuries. The early modern period is long enough to encompass observable trends in economy and society and cohesive enough to have a traditional name—the Old Regime, encouraging the hope that any model which emerges from the available evidence will be valid for the period as a whole, well defined in time, and characterized by duration.<sup>7</sup>

THE ECOLOGICAL DIVERSITY OF THE SPANISH LANDSCAPE makes it difficult to approach the topic. Some 68 percent of the surface area is "dry" Spain, receiving less than 500 millimeters of rain a year—in most areas, far less. The rest is "wet" Spain, including the north and northwest coasts and mountainous areas in the northeast and south. Coastal areas are well supplied with good natural ports, but inland transport faces extraordinary obstacles. For all practical purposes, the rivers are not navigable. They descend precipitously from their headwaters to the sea, varying enormously in seasonal flow. Their steep banks and a propensity to deposit silt from flooding severely limited their use for irrigation until the twentieth century. Most of the surface area of Spain is not suitable for cultivation. Formidable mountain chains divide and isolate historical regions from one another, with connections only through a handful of passes and natural defiles.<sup>8</sup> Overall, Spain's natural resources present more challenges than opportunities and make its rise as a world power all the more impressive.

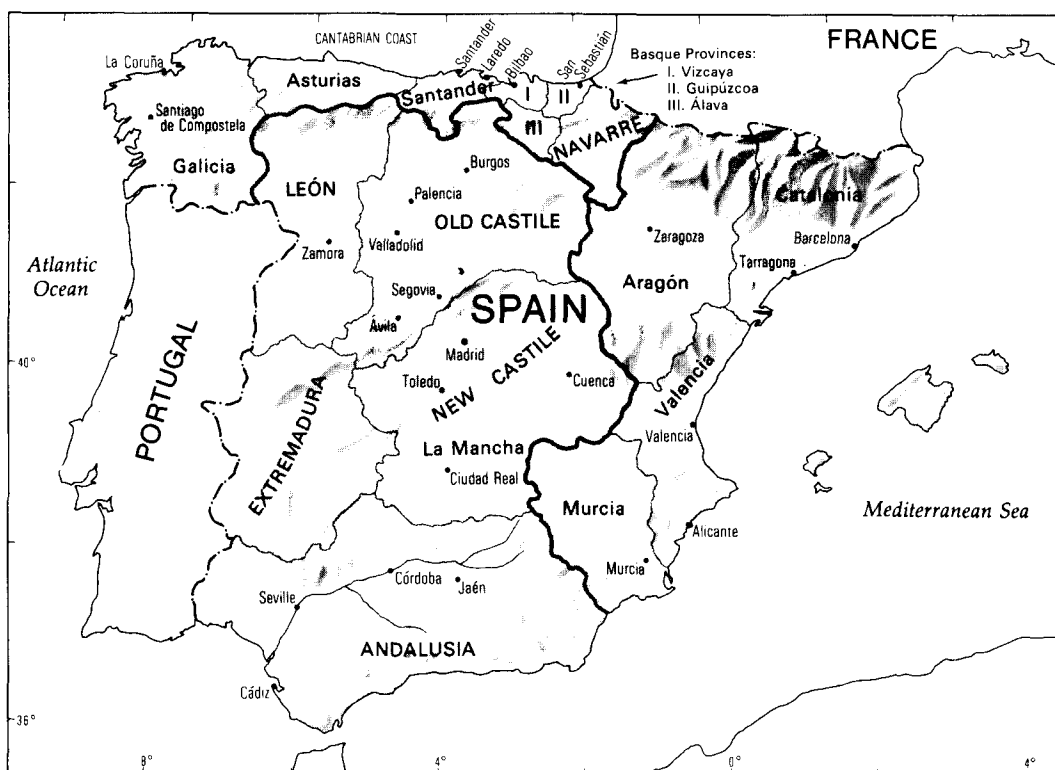
Developed in separate enclaves, every geographical region of Spain had its own mixture of ecological, social, and legal variables that influenced the local economy and the potential income of its inhabitants. No general scheme can encompass every local variation, but it can provide a comparative framework to understand

<sup>5</sup> Recent Spanish historical demography relies on French and English precursors, such as Michel Fleury and Louis Henry, *Nouveau manuel de dépouillement et d'exploitation de l'état civil ancien* (Paris, 1965); Louis Henry, *Manuel de démographie historique* (Paris, 1967); T. H. Hollingsworth, *Historical Demography* (Ithaca, N.Y., 1969); and D. V. Glass and D. E. C. Eversley, eds., *Population in History* (London, 1965). To date, most of the published work consists of what is called aggregative analysis, focusing on censuses and vital events and tracing them through time. The analysis of individual behavior through the reconstitution of families has barely begun. The Instituto Nacional de Estadística in Madrid has recently published several Spanish censuses from the late sixteenth through the late eighteenth century.

<sup>6</sup> Marc Bloch, *French Rural History: An Essay on Its Basic Characteristics*, Janet Sondheimer, trans. (Berkeley and Los Angeles, 1966), xxiii.

<sup>7</sup> Fernand Braudel, "History and the Social Sciences," in *Economy and Society in Early Modern Europe: Essays from Annales*, Peter Burke, ed. (New York, 1972).

<sup>8</sup> Vicens, *Economic History of Spain*, 9–20. See also the pioneering work of Román Perpiñá, "De naturaleza: La Infraestructura económica," *Anales de economía*, 5 (1945): 401–36; John Salyer, "La Política económica de España en la época del mercantilismo," *Anales de economía*, 8 (1948): 303–27; Maurice Schwartzmann, "Background Factors in Spanish Economic Decline," *Explorations in Entrepreneurial History*, 3 (1951): 221–47.



Spain Divided into Three Regions: Inland Castile and Andalusia, North and Northwest Coasts, East and Southeast Coasts (map prepared by Gregory Chu of the Cartography Laboratory at the University of Minnesota).

those variations. To analyze the patterns that have emerged from local studies, I will consider Spain as three distinct regions: inland Castile, Andalusia and the southwest coast; the north and northwest coasts and their hinterlands; and the east and southeast coasts and their hinterlands (see map). The regions thus defined have certain characteristics in common, even though they exhibit an internal diversity that increases as the focus narrows.

Within the matrix created by the ecological, social, and legal features of a region, the central economic relationship was between population and resources. From the admittedly partial evidence now available, it appears that a particular pattern of demographic behavior characterized each region, developed in response to the resources available to sustain its inhabitants.<sup>9</sup> These patterns were already firmly

<sup>9</sup> The first major collection of new work on historical demography in Spain was the *Actas de las I jornadas de metodología aplicada de las ciencias históricas: Metodología de la historia moderna: Economía y demografía* (hereafter, *Actas*) (Santiago de Compostela, 1975). See also Bernard Vincent, "Récents travaux de démographie historique en Espagne (XIV<sup>e</sup>–XVIII<sup>e</sup> siècles)," *Annales de démographie historique* (1977): 463–91; and Manuel Martín-Galán, "Fuentes y métodos para el estudio de la demografía histórica castellana durante la Edad Moderna," *Hispania*, 41 (1981): 1–231. The Asociación de Demografía Histórica (ADEH), founded in Madrid in 1982, has quickly grown to several hundred members, proof of the interest in population studies in Spain.

Marriage behavior is perhaps the most crucial demographic variable. See E. A. Wrigley, "The Prospects for Population History," *Journal of Interdisciplinary History*, 12 (1981): 207–26. Older assumptions that Spanish marriage and fertility differed significantly from that of Northern Europe were based on very little data, compared to the model developed by John Hajnal, "European Marriage Patterns in Perspective," in Glass and Eversley, *Population in History*, 101–43. The few reconstitution



in place by the sixteenth century, and they would endure through the early modern period as a whole. Although the economic experiences of the three regions differed, they are variations on a single economic model.

The first region—inland Castile, plus Andalusia and the southwest coast—is the best documented and the obvious place to begin. It encompassed most of the surface area of Spain, even though fragmented by mountain ranges, and contained the vast majority of the early modern population (see Tables 1–3). Some of the best arable land lies in areas of unpredictable rainfall. From year to year, the harvest volume depended heavily on the weather in the dry-farming areas of Old and New Castile. Yet these same areas could produce very well under the right circumstances, and it is misleading to equate “dry” Spain with poor agricultural production. Some areas regularly produced grapes, olives, and orchard crops in addition to grain.<sup>10</sup> Of the land not suitable for crops on a regular basis, some was used as pasture for local animals and for transhumant flocks. Herding was especially prevalent in New Castile and Extremadura, where several hundred thousand animals spent the winter, returning north for the summer. Traditional historiography has stressed the conflict between farming and herding, but it is better understood as a mutually profitable, if contentious, partnership.

Human settlement tended to be concentrated rather than scattered, a result of the location of fresh water supplies and the need for defense on the medieval frontier between Christian and Muslim forces. The kingdom of Castile (including Andalusia) formed one single customs area, surrounded by a ring of inland customs ports (*puertos secos*). Geographical and fiscal divisions and the difficulty of inland transport isolated much of Castile from long-distance trade, but at the regional level there were numerous networks of exchange. A small number of powerful cities acted as centers of manufacturing and trade, the most important among them securing their supplies from large hinterlands and providing links to the port towns on the coasts. Much of the political power in Spain resided in the eighteen cities and towns of central Castile represented in the parliament (*Cortes*).<sup>11</sup> Along with sharing the fruits of power, such as possession of the American colonies, Castile also bore the heaviest tax burden in Spain.

The social structure in Castile and Andalusia featured a landowning nobility that enjoyed considerable prestige and status but with wide variations in wealth and national prominence. There was room for mobility at all levels of society. In many areas of Castile, ownership of land was fairly common, although landless day-laborers made up the majority of many farming communities. A few

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studies now available show a considerable range of marriage behavior in Spain, closely linked to regional economic realities. See Vicente Pérez-Moreta, “Matrimonio y familia: Algunas consideraciones sobre el modelo matrimonial español en la edad moderna,” *Boletín de la ADEH*, 4 (March 1986): 3–51; and Angeles Valero Lobo, “Edad media de acceso al matrimonio en España: Siglos XVI–XIX,” *Boletín de la ADEH*, 2 (July 1984): 39–48, for summaries of the evidence. Hervé le Bras and Emmanuel Todd, “Mountains, Rivers and the Family: Comments on a Map from the 1975 French Census,” *The World We Have Gained: Histories of Population and Social Structure*, Lloyd Bonfield, et al., eds. (Oxford, 1986), 379–87, discusses both the antiquity and the persistence of regional demographic variations in France.

<sup>10</sup> Alain Huetz de Lempis, *Vignobles et vins du nord-ouest de l'Espagne*, 2 vols. (Bordeaux, 1967).

<sup>11</sup> Domínguez Ortiz, *Antiguo Régimen*, 79–87.

**TABLE 1**  
**Population Growth in the Sixteenth-Century Kingdom of Castile**

	Taxpaying Householders			Total No. of Households in 1591	Percent of Castile	
	in 1528	in 1591	Percent Increase		in 1528	in 1591
Northwest (7 areas)	67,039	124,908	86.3	183,771	7.8	13.9
Old Castile (13 areas)	300,446	368,200	22.6	443,817	35.0	33.6
New Castile and Extremadura (14 areas)	208,633	360,314	72.7	394,706	24.3	29.8
South and Southeast (5 areas)	154,366	230,650	49.4	249,550	18.0	18.9
Granada*	126,469 (280,835)			50,448 (299,998)	14.8 (32.8)	3.8 (22.7)
TOTAL	<hr/> 856,953			<hr/> 1,322,292		
TOTAL PERSONS (households × 5)				6,611,460		
* Granada was a special case. After a rebellion in 1570–71, about 15,000 households were relocated in Castile.						

SOURCE: Annie Molinié-Bertrand, *Au siècle d'or: L'Espagne et ses hommes: La Population du Royaume de Castille au XVI<sup>e</sup> siècle* (Paris, 1985), 308–09, which lists thirty-nine administrative areas, without grouping them by region.

small-scale merchants sufficed to carry on trade in small urban centers, but in the larger cities—Burgos, Segovia, Toledo, Córdoba, Seville—merchant organizations controlled the most important commerce. The old saw about the lack of a native merchant community in Castile is simply not true. In Burgos, the merchants were nearly all Spaniards, whereas in Seville, there was a large admixture of Italians and other foreigners, many of whom established permanent residence and acquired Spanish citizenship.<sup>12</sup>

<sup>12</sup> Manuel Basas Fernández, *El Consulado de Burgos en el siglo XVI* (Madrid, 1963); Ruth Pike, *Aristocrats and Traders: Sevillian Society in the Sixteenth Century* (Ithaca, N.Y., 1972); and Ruth Pike, *Enterprise and Adventure: The Genoese in Seville and the Opening of the New World* (Ithaca, N.Y., 1966).

TABLE 2

## Population Totals in Late Sixteenth-Century Spain

	Population	Percent of Total
CASTILE (1591)	6,611,460	82.5
CROWN OF ARAGON		
Aragón (1603)	354,920	
Catalonia (1553)	326,970	
Valencia (1563)	320,375	
Balears (1583)	<u>130,000</u>	
Subtotal	1,132,265	14.1
BASQUE PROVINCES		
Navarre (1553)	138,748	
Guipúzcoa (1553)	69,665	
Alava (1559)	<u>60,696</u>	
Subtotal	269,109	3.4
TOTAL for Spain	8,012,834	

SOURCE: Molinié-Bertrand, *Siècle d'or*, 308–09.

TABLE 3

Population Totals in the Late Sixteenth Century,  
Rearranged by Broad Regions

	Inhabitants	Percent
Old Castile, New Castile, Extremadura, and Andalusia	5,469,910	68.3
North and Northwest	1,266,294	15.8
Crown of Aragón and Murcia	<u>1,276,630</u>	15.9
TOTAL INHABITANTS	8,012,834	

SOURCE: Molinié-Bertrand, *Siècle d'or*, 308–09.

Legally, Castile was probably more closely controlled by the crown than were the other regions of the peninsula, but there was considerable local jurisdiction over tax collection, the use of municipally held lands, and other matters that directly affected the ability of the local population to make a living.<sup>13</sup> Land generally passed to a single heir, and the upper classes of society often arranged for legal entails to prevent the dispersal of their property. The average size of landholdings tended to be smaller in the north and larger in the center and south of Castile, partly because of the quality of the soil and partly as a legacy of the medieval *Reconquista*, when land recaptured from the Muslims was turned over to the military aristocracy and town councils for safeguarding. These characteristics shaped the economic experience of the region in the early modern period.

Published work is still too sparse to generalize with certainty about demographic behavior in Castile, but it would appear that the pattern during the Habsburg centuries included a substantial rate of celibacy, a younger age at marriage than elsewhere in Spain, and high marital fertility. Among Moriscos (converts from Islam to Christianity and their descendants), the age at marriage and the celibacy rate were lower, and the fertility rate was higher.<sup>14</sup> The death rate surpassed the birth rate in hard times, but, with favorable harvest conditions, the population could and did grow. The average population density in most of Castile was low compared to Italy, France, and other areas in Spain, rarely exceeding twenty inhabitants per square kilometer during the Habsburg period. In areas such as La Mancha, the density was lower still.<sup>15</sup>

Increasing evidence attests to an impressive population rise in Castile and Andalusia during the sixteenth century, beginning as early as 1450.<sup>16</sup> This rise

<sup>13</sup> Domínguez Ortiz, *Antiguo Régimen*, 194–220.

<sup>14</sup> Higher Morisco fertility is shown indirectly by the high proportion of Morisco households headed by married couples (88 percent), compared to 68.5 percent among Old Christians in the city of Ciudad Real in 1586. Female heads of households (widows and spinsters) were 2.6 times more numerous among Old Christians than among Moriscos. There are several possible reasons for this pattern, among them that Morisco men were not taken into the military, rarely entered the church, and found their movements restricted by the government. Archivo General de Simancas, Expedientes de Hacienda, legajo 83. The raw data have been published by Jerónimo López-Salazar, "Estructura socioprofesional de Ciudad Real en la segunda mitad del siglo XVI," *20,000 km<sup>2</sup>* (Ciudad Real, Autumn–Winter 1977): 51–92. His compiled tables should be used with care, due to numerous errors of transcription and mathematics. See also Bernard Vincent, "Les Morisques d'Extremadure au XVI<sup>e</sup> siècle," *Annales de démographie historique* (1974): 438–39.

<sup>15</sup> Domínguez Ortiz, *Antiguo Régimen*, 73–77.

<sup>16</sup> Annie Molinié-Bertrand, *Au siècle d'or: L'Espagne et ses hommes: La Population du Royaume de Castille au XVI<sup>e</sup> siècle* (Paris, 1985). This French doctoral thesis analyzes several detailed censuses from the sixteenth century. See also Miguel Angel Ladero Quesada, *España en 1492* (Madrid, 1978), 29–34; Alvaro Castillo, "Population et 'richesse' en Castille durant la seconde moitié du XVI<sup>e</sup> siècle," *Annales: économies, sociétés, civilisations*, 20 (July–August 1965); Valentina Fernández Vargas, *La Población de León en el siglo XVI* (Madrid, 1968); Guillermo Herrero Martínez de Azcoitia, *La Población palentina en los siglos XVI y XVII* (Palencia, 1961); María Carmen González Muñoz, *La Población de Talavera de la Reina (siglos XVI–XX): Estudio socio-demográfico* (Toledo, 1975); Juan Gutiérrez Nieto, "Evolución demográfica de la Cuenca de Segura en el siglo XVI," *Hispania*, 29 (1969); Jerónimo López-Salazar Pérez, "La Población manchega en los siglos XVI y XVII," *Revista internacional de sociología*, 39 (January–March 1981): 7–31; and (April–June 1981): 193–231; María Dolores Marcos González, *Castilla la Nueva y Extremadura* (Salamanca, 1971); Jean-Pierre Molénat, "Tolède et ses finages du temps des Rois Catholiques: Contribution à l'histoire sociale et économique de la cité avant la révolte des Comunidades," *Mélanges de la Casa de Velázquez*, 8 (1972): 327–77; Francisco Morales Padrón, *Historia de Sevilla: III: La Ciudad del quinientos* (Seville, 1977); Hipólito Sancho de Sopranis, "Estructura y perfil demográfico de Cádiz



marked the recovery of the population from the depression of the fourteenth and early fifteenth centuries, which had been exacerbated but not caused by the Black Death. Official counts of households, documenting the sixteenth-century rise, have been corroborated for many parts of Castile by parish registers and other local records.<sup>17</sup> The strongest increases seem to have occurred during the middle third of the century, although the timetable has been variously estimated.<sup>18</sup> For the region I have defined, growth was strongest in New Castile, weaker in Andalusia, and weakest in Old Castile and León (see Table 1). The cause of this growth in population is not clear, although it is logical to think that it simply resulted from several decades of good weather, generally good harvests, and a lack of serious epidemics.<sup>19</sup>

As the population rose, and internal and external demand for food followed, a great deal of land was plowed that had not been farmed in recent memory.<sup>20</sup> Evidence exists of market stimulation in the increasing amount of land devoted to grapes and olives, especially near the growing cities and the southern ports leading to the New World. With low population density, considerable availability of land, and minimal technology, an extension of cultivation using traditional techniques was the logical response to population growth in Castile.<sup>21</sup> Some of the agricultural expansion came at the expense of common grazing land and the traditional rights-of-way and grazing privileges for transhumant animals. Nu-

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en el siglo XVI," *Estudios de historia social de España* (1952): 533–613; Serafin de Tapia, "Las Fuentes demográficas y el potencial humano de Ávila en el siglo XVI," *Cuadernos abulenses* (July–December 1984): 31–88; and Angel Rodríguez Sánchez, *Cáceres: Población y comportamientos demográficos en el siglo XVI* (Cáceres, 1977).

<sup>17</sup> Most of the works cited here used parish registers to one degree or another.

<sup>18</sup> Around Segovia, growth was most marked in 1540–70. Angel García Sanz, *Desarrollo y crisis del Antiguo Régimen en Castilla la Vieja: Economía y sociedad en tierras de Segovia 1500–1814* (Madrid, 1977), 55. In Old Castile as a whole, growth was most apparent between the population counts of 1561 and 1584. Bartolomé Bennassar, *Valladolid au siècle d'or: Une Ville de Castille et sa campagne au XVI<sup>e</sup> siècle* (Paris, 1967), 171–72. The city of Ciudad Real showed a strong rise from about 1525 to about 1575. Carla Rahn Phillips, "Ciudad Real no período dos Habsburgos: Um Estudo demográfico," *Anais de historia*, 7 (December 1975): 151–65; Carla Rahn Phillips, *Ciudad Real, 1500–1750: Growth, Crisis, and Readjustment in the Spanish Economy* (Cambridge, Mass., 1979), 22. Seville grew rapidly in 1540–80. See Pike, *Aristocrats and Traders*, 10–14; Jean Sentaurens, "Séville dans la seconde moitié du XVI<sup>e</sup> siècle: Population et structures sociales: Le Recensement de 1561," *Bulletin hispanique*, 77 (July–December 1975): 321–90. Córdoba and its surrounding area experienced general growth from 1530 to 1570–80. See José Ignacio Fortea Pérez, *Córdoba en el siglo XVI: Las Bases demográficas y económicas de una expansión urbana* (Córdoba, 1980), 116, 142; José Manuel Cuenca Toribio, *Andalucía: Una Introducción histórica* (Córdoba, 1980); and Bartolomé Yun Casalilla, *Crisis de subsistencia y conflictividad social en Córdoba a principios del siglo XVI* (Córdoba, 1980).

<sup>19</sup> Continuing work on climate and history discounts any mechanistic relationship between them. At the same time, it reinforces the notion that year-to-year harvest volume, affected by the weather, had a crucial influence on demographic and economic trends. *Climate and History: Studies in Interdisciplinary History*, Robert I. Rotberg and Theodore K. Rabb, eds. (Princeton, N.J., 1981); Emmanuel Le Roy Ladurie, *Times of Feast, Times of Famine: A History of Climate since the Year 1000*, Barbara Bray, trans. (New York, 1971), 232–43. Voluminous records exist for detailed analysis of weather-related phenomena in early modern Spain, although little of it has been analyzed to date. See Bennassar, *Valladolid*, 39–51.

<sup>20</sup> Ladero Quesada, *España en 1492*, 68–69; David Vassberg, *Land and Society in Golden Age Castile* (Cambridge, 1984), 151–83.

<sup>21</sup> Ester Boserup, *The Conditions of Agricultural Growth: The Economics of Agrarian Change under Population Pressure* (London, 1965); Isabel Moll Blancas, "Forward from Malthus: The State of Population Theory in 1981," *Boletín de la ADEH*, 2 (November 1984): 28–38.

merous legal cases document the growing tension between herders and farmers as the sixteenth century progressed, with herding literally losing ground as crop lands expanded.<sup>22</sup> Despite the pressure from farming and rising costs for seasonal pasture, wool production seems, nonetheless, to have increased, at least until the middle years of the century.<sup>23</sup> This increase reinforces the idea that generally favorable weather underlay expansion in the Castilian countryside.

Market stimulation, both internal and from the New World, led to a substantial growth in Castilian industry, particularly textile manufacturing but also metallurgy, paper production, soapmaking, and fish salting, all of which required considerable investment. Some of the capital, researchers are beginning to discover, came from profits made in local and international trade.<sup>24</sup> The merchants of Castile and Andalusia benefited from the sixteenth-century boom as much or more than did any other group, profiting from trade with Northern Europe and the New World. Seville presided over the explosive growth in New World trade.<sup>25</sup> Burgos and its merchant council (*Consulado*) controlled the northern wool trade, enjoying its greatest prosperity in the middle third of the sixteenth century.<sup>26</sup> In Segovia, many noble families as well as merchants were attracted to the commerce in wool and the manufacture of cloth, consolidating their fortunes and their social station at the same time, and finding no contradiction therein.<sup>27</sup> Many merchants endowed charities and pious foundations with part of their fortunes, making the sixteenth century one of the great eras for charitable endowments in Castile.<sup>28</sup>

Growth was not free, as the people of Castile came to realize. Prices rose throughout the sixteenth century, stimulated at first by the internal rise in population and demand, then by the continuing arrival of treasure from the New

<sup>22</sup> Julius Klein, *The Mesta: A Study in Spanish Economic History* (Cambridge, Mass., 1920).

<sup>23</sup> Jean-Paul Le Flem, "Las Cuentas de la Mesta, 1510–1709," *Moneda y crédito*, 121 (1972): 29–30, and tables; Enrique Llopis Agelán, "Crisis y recuperación de las explotaciones trashumantes: La Cabaña del Monasterio de Guadalupe (1579–1679)," *Investigaciones económicas*, 13 (September–December 1980): 144; Carla Rahn Phillips, "The Spanish Wool Trade, 1500–1780," *Journal of Economic History*, 42 (December 1982): 778–83.

<sup>24</sup> Fortea, *Córdoba*, 286–88, 335–78; Paulino Iradiel Murugarren, *Evolución de la industria textil castellana en los siglos XIII–XVI: Factores de desarrollo, organización y costes de la producción manufacturera en Cuenca* (Salamanca, 1974), 99–103, 110–18, 217–50. Spanish economic historians have also turned their attention to the connection between agriculture and industrial development, entering the broader debate over protoindustrialization in early modern Europe. Session on "Protoindustrialización" at the II Congreso de Historia Económica, University of Alcalá de Henares, 17–19 December 1981, published in the *Revista de historia económica*, 2 (Autumn 1984): 11–146.

<sup>25</sup> Pierre Chaunu and Huguette Chaunu, *Séville et l'Atlantique*, 8 vols. in 12 (Paris, 1955), abridged as *Séville et l'Amérique aux XVI<sup>e</sup> et XVII<sup>e</sup> siècles* (Paris, 1977). The major points are summarized in "Atlantic Economy," *Essays in European Economic History 1500–1800*, Peter Earle, ed. (Oxford, 1974), 119–23.

<sup>26</sup> Joseph Pérez, *La Revolución des "comunidades" de Castille (1520–1521)* (Bordeaux, 1970), 40; Nazario González, *Burgos, ciudad marginal de Castilla: Estudio de geografía urbana* (Burgos, 1958); Teófilo López Mata, *La Ciudad y castillo de Burgos* (Burgos, 1949); Basas Fernández, *Consulado*; Manuel Basas Fernández, "La Función del consulado de Burgos en el apogeo económico de Castilla," in *La Ciudad de Burgos: Actas del Congreso de Historia de Burgos: MC aniversario de la fundación de la ciudad, 884–1984* (León, 1985), 233–43; Benito Royuela Rico, "Una Aproximación a la demografía burgalesa: Las Relaciones parroquiales de 1563–1564," in *Ciudad de Burgos*, 271–92.

<sup>27</sup> García Sanz, *Segovia*, 82; Eduardo Martínez de Pisón, *Segovia: Evolución de un paisaje urbano* (Madrid, 1976), 65.

<sup>28</sup> William A. Christian, Jr., *Local Religion in Sixteenth-Century Spain* (Princeton, N.J., 1981), 106.

World.<sup>29</sup> Despite external wars, emigration, privation, disease, and high celibacy, population continued to rise.<sup>30</sup> By the late sixteenth century, population growth and rising prices upset the balance of the Castilian economy. A government survey in 1575–78 provides evidence of overpopulation in New Castile, reporting the exhaustion of firewood, the plowing of formerly common land, and the insufficiency of pasture. It seems clear that Castile was approaching a crisis during the 1570s before a series of harvest disasters struck.<sup>31</sup> Parish registers throughout Castile show a weakening of the population rise by 1580 at the latest and earlier in largely rural areas that had already reached the limits of their resources. Available evidence reveals that emigration from these rural areas accounted for some of their population losses and for the postponement of absolute population declines in other areas until the end of the century.<sup>32</sup> Nonetheless, the best estimate is that the decline in Castilian population began about 1580, when a crisis in the rural economy brought the sixteenth-century population rise to an end.<sup>33</sup>

THE GENERAL PRINCIPLES LAID OUT IN 1798 by Thomas Malthus placed population change at the center of economic change.<sup>34</sup> In his analysis, population tended to increase toward the limits of subsistence. These limits were influenced by the

<sup>29</sup> Earl J. Hamilton is the scholar most often associated with the theory that the increasing of money caused the price rise, although he did consider other potential causes as well. *American Treasure and the Price Revolution in Spain, 1501–1650* (1934; rpt. edn., New York, 1965), 53–64, 283–306. See also J. H. Elliott, *The Old World and the New, 1492–1650* (Cambridge, 1970), 59–78; Harry A. Miskimin, "Population Growth and the Price Revolution in England," *Journal of European Economic History*, 4 (Spring 1975): 179–86; Modesto Ulloa, "Castilian Seigniorage and Coinage in the Reign of Philip II," *Journal of European Economic History*, 4 (Fall 1975): 459–80; Modesto Ulloa, *La Hacienda real de Castilla en el reinado de Felipe II*, 2d edn. (Madrid, 1978), 841–44.

<sup>30</sup> Braudel, *Mediterranean*, 2: 740–43; Linda Martz, *Poverty and Welfare in Habsburg Spain* (Cambridge, 1983), 113–19; Jaime Carrera Pujal, *Historia de la economía española*, 5 vols. (Barcelona, 1943–47), 1: 309; T. Egido López, "Aportación al estudio de la demografía española: Los Niños espósitos de Valladolid (siglos XVI–XVIII)," *Actas*, 333–46.

<sup>31</sup> José Gentil da Silva, *Desarrollo económico, subsistencia y decadencia en España* (Madrid, 1967); Noël Salomon, *La Campagne de Nouvelle Castille à la fin du XVI<sup>e</sup> siècle: D'Après les "Relaciones topográficas"* (Paris, 1964); *Relaciones histórico-geográfico-estadísticas de los pueblos de España hechas por iniciativa de Felipe II: Ciudad Real*, Carmelo Viñas y Mey and Ramón Paz, eds. (Madrid, 1971); Braudel, *Mediterranean*, 1: 575; Vassberg, *Castile*, 153–58; Silva, *España*, 193–94, 208; Hamilton, *American Treasure and the Price Revolution in Spain*, 247–54.

<sup>32</sup> Felipe Ruiz Martín, "La Población española al comienzo de los tiempos modernos," *Cuadernos de historia: Anexos de la Revista Hispania*, no. 1 (Madrid, 1967): 197.

<sup>33</sup> García Sanz, *Segovia*, 52–53, discusses the parish registers of twenty-seven population centers in the Segovia area that showed stagnation or decline from the mid-sixteenth century. Vicente Pérez-Moreda, *Las Crisis de mortalidad en la España interior: Siglos XVI–XIX* (Madrid, 1980), 246, cites and discusses numerous local studies supporting the idea of a demographic crisis from the 1570s and 1580s. See also François Brumont, "L'Évolution de la population rurale durant le règne de Philippe II: L'Exemple du Nord-Ouest de la Vieille Castille," *Mélanges de la Casa de Velázquez*, 14 (1978): 260–68; M. de Castro Matia, "Los Libros de cuentas de la 'fábrica' de las iglesias parroquiales: El Ejemplo de Fuentes de Don Bermudo en Tierra de Campos," *Actas*, 157–68; Jean-Paul Amalric and François Brumont, "Evolución de las estructuras agrarias en la Castilla moderna: El ejemplo de la Bureba," *Actas*, 231–36; Manuel Fernández Álvarez, "La Demografía de Salamanca en el siglo XVI a través de los fondos parroquiales," *Actas*, 287; Alberto Marcos Martín, *Auge y declive de un núcleo mercantil y financiero de Castilla la Vieja: Evolución demográfica de Medina del Campo durante los siglos XVI y XVII* (Valladolid, 1978), 251–53; and J. H. Elliott, "Yet Another Crisis?" in *The European Crisis of the 1590s*, Peter Clark, ed. (London, 1985), 301–12.

<sup>34</sup> Thomas Robert Malthus, *An Essay on the Principle of Population*. See also William Petersen, *Malthus* (Cambridge, Mass., 1979); J. Dennis Willigan and Katherine A. Lynch, *Sources and Methods of Historical Demography* (New York, 1982), 20–31.

structures of landholding, rents, taxes, and other exactions, but the motor of change was demographic. Karl Marx provided an alternative model in the mid-nineteenth century. For Marx, the structures of landholding, rents, taxes, and other exactions—not population—lay at the center of economic analysis.<sup>35</sup> Many of the authors whose research is discussed here cast their findings within a Marxian model, yet the extent to which the growing distress of sixteenth-century Castile was the result of an increase in seignorial pressure is still the subject of much debate. The amount of growth in the sixteenth century and the timing and severity of the subsequent crisis in population varied from place to place, depending on local conditions. Seignorial jurisdiction, or the lack of it, was only one of those conditions. A rural hill town in Extremadura with poor soil, seignorial jurisdiction, and limited access to markets would have quickly reached a ceiling in growth. A rich, independent town on the Andalusian plains could have grown much further.

Andalusia was known for its vast estates and powerful nobility. It is not yet clear how this pattern affected the timetable of growth and crisis in the sixteenth century, or who benefited most from the possibilities for commercial agriculture. The general growth of the local economy beginning in the late fifteenth century is well established, however. Andalusia was able to export grain in the early sixteenth century, supplying the Indies fleets and neighboring Portugal with its surplus. Even so, there were limits to Andalusian growth. Some time before 1570, Andalusia changed from an exporter of grain to a habitual importer, an event that is usually taken to mean a crisis in production. It also meant that the region around Seville could afford to import grain, in exchange for its surpluses of oil, wine, and wealth from the New World. A flexible system of food supply was possible for a port town with a rich agricultural hinterland.<sup>36</sup>

Cities and towns less fortunate than Seville were unable to compensate for their limited local resources. Thus the decline of industry and urban centers followed on the heels of agricultural crisis. Small workshops predominated in the Castilian textile industry as they did elsewhere in Europe. From the late fifteenth century on, specialization and dispersion of cloth production increased, often involving up to a dozen guilds and employing workers outside the guilds for less skilled jobs. The Segovian woolen industry provides a good example of this evolution, starting as a small-scale domestic industry producing mostly coarse cloth in about the year

<sup>35</sup> Karl Marx's concept of historical change is most clearly expressed in his and Fredrick Engels' *German Ideology*, Part I (1845–46). See Pierre Vilar, "The Age of Don Quixote," in Earle, *Essays in European Economic History*, 100–13, for a Marxist interpretation of the decline of Spain. Guy Bois, *Crise de féodalisme* (Paris, 1976); and Peter Kriedte, *Peasants, Landlords and Merchant Capitalists: Europe and the World Economy, 1500–1800* (Cambridge, 1983), represent a new trend that attempts to reconcile the approaches of Marx and Malthus.

<sup>36</sup> Ladero Quesada, *España en 1492*, 97–99; Carmelo Viñas y Mey, *El Problema de la tierra en la España de los siglos XVI–XVII* (Madrid, 1941), 32–53; Vassberg, *Castile*, 204–10; Salomon, *Nouvelle Castille*, 249–50; Helen Nader, "Noble Income in Sixteenth-Century Castile: The Case of the Marquises of Mondéjar, 1480–1580," *Economic History Review*, 2d ser., 30 (1977): 412–28; Braudel, *Mediterranean*, 1: 584–91; Pierre Ponsot, "En Andalousie occidentale: Les Fluctuations de la production du blé sous l'ancien régime," *Etudes rurales*, 34 (1969): 97–112. Maurice Aymard, *Venise, Raguse et le commerce du blé pendant la seconde moitié du XVI<sup>e</sup> siècle* (Paris, 1966), analyzes a similar situation in the eastern Mediterranean.

1500 and becoming the most famous center of Castilian fine cloth in the late sixteenth century. At the zenith of the Segovian industry, *circa* 1560–90, it produced some 13,000 cloths annually, most of them in the city; rural workers were employed mostly for washing and spinning raw wool. Rural villages near Segovia continued to produce low quality cloth, however, and the population of the area was heavily employed in textile manufacturing. The industry peaked by about 1590, then began to decline, dragged down by the surrounding agricultural crisis. As population outstripped food production, rising agricultural prices slowly squeezed out demand for manufactures.<sup>37</sup>

Contemporary and modern observers alike have blamed foreign competition and the export of fine wool for Castile's industrial decline. Others have cited rising labor costs, government regulation, and guild control. Still others have blamed merchants who arranged for the manufacture and sale of cloth, because they turned away from industry at a crucial time. None of these conditions made the situation any easier, to be sure, but they seem to have been symptoms more than causes of industrial decline. A large city could insulate itself somewhat from a surrounding rural crisis by coercing its subordinate villages into granting more favorable terms for food, and credit could sustain urban industry and trade for a time.<sup>38</sup> Coercion and credit may explain why industrial prosperity in Segovia and Toledo, for example, seems to have outlasted agricultural prosperity in those regions by ten to twenty years. In Ávila, Palencia, Zamora, Ciudad Real, and other centers, industry did not decline until the early seventeenth century.<sup>39</sup>

In contrast, Cuenca and Córdoba experienced manufacturing declines in the last quarter of the sixteenth century. Industrial growth in Córdoba had largely been a result of the investment of commercial profits by native and foreign merchants eager to share in the prosperity generated by increased demand during the early sixteenth century. As in Segovia, the cloth makers of Córdoba had improved the quality of their product since the late fifteenth century, widened their market network, and benefited from increased governmental regulation of the industry. But Córdoba was not a center of fine cloth production like Segovia. Instead, it was a center for the finishing and sale of cloth woven in an extended region and for the merchants who provided the cash, credit, and organization for rural manufacturing. With production relying on rural areas, the city of Córdoba could not postpone the effects of the agricultural crisis, and local merchants attempting to sustain their own incomes made the situation worse by raising credit rates and exploiting rural workers. Merchants gradually withdrew their invest-

<sup>37</sup> See Felipe Ruiz Martín, "Un Testimonio literario sobre las manufacturas de paños en Segovia por 1625," *Homenaje al Profesor Alarcos*, 2 vols. (Valladolid, 1966), 5–7, for Segovian production figures. The mechanism of industrial crisis is implicit in many local studies, but it is explained with particular clarity by García Sanz, *Segovia*, 58.

<sup>38</sup> Ruggiero Romano, "Between the Sixteenth and Seventeenth Centuries: The Economic Crisis of 1619–22," in *The General Crisis of the Seventeenth Century*, N. Geoffrey Parker and Lesley M. Smith, eds. (London, 1978), 202.

<sup>39</sup> García Sanz, *Segovia*, 59. David Ringrose, "The Impact of a New Capital City: Madrid, Toledo, and New Castile, 1560–1660," *Journal of Economic History*, 33 (December 1973): 761–91; Michael Weissner, "The Decline of Castile Revisited: The Case of Toledo," *Journal of European Economic History*, 2 (Winter 1973): 614–40; Phillips, *Ciudad Real*, 50–56.



ment and expertise from the textile industry.<sup>40</sup> It is tempting to blame these merchant entrepreneurs for failing to innovate when they were still actively involved in manufacturing (assuming they could have avoided the guilds), but it is not clear how they could have done so. Local merchants had no way to solve the underlying difficulty of a weak internal market, held back by ecological and technological limits on agriculture.

Historians often accuse the Spanish merchant community of lacking the skills and the entrepreneurial outlook of merchants in other nations, but they are beginning to realize the emptiness of that charge. In Segovia and Córdoba, and undoubtedly elsewhere, the merchant communities provided not only investment capital but also the entrepreneurial skills that fostered the growth of manufacturing. It is a measure of their skill that many merchants backed away from unprofitable investments before suffering ruin.<sup>41</sup> Whether they moved into finance, landownership, officeholding, or other business ventures, they were astute in salvaging what they could from the unfavorable situation of the late sixteenth century. They were not in business to be either defectors or heroes but to survive.

Scholars primarily interested in prices, trade, and the flow of bullion from the New World have often favored a timetable that pushes the onset of Spain's decline forward into the seventeenth century, even as far as 1650, though doing so ignores clear signs of earlier difficulties in the economy as a whole.<sup>42</sup> Expansion in American trade and bullion flows seems to have lasted from 1504 to about 1610 and was especially notable in 1504–50 and 1562–92. Even though 1608 saw the peak in shipping volume and silver returns, stagnation and signs of decay appeared from 1593 to 1610.<sup>43</sup> A crisis in industry, trade, and credit affecting much of Europe followed in 1619–21, although the connections between this crisis and the downturn of American trade remain to be demonstrated.<sup>44</sup>

We still know too little to judge the extent to which bullion affected Castilian developments. Some historians hint that Castile could have avoided crisis and decline if New World wealth had been invested in the economy rather than in imperial wars. It is not clear, however, what amount of bullion the Castilian economy could have absorbed. Heavier investment might have made the situation worse instead of better, adding to inflation without adding substantially to production.

<sup>40</sup> Fortea, *Córdoba*, 267–307, 358, 393–94, 469–75; Iradiel Murugarren, *Evolución de la industria textil*, 209.

<sup>41</sup> Braudel, *Mediterranean*, 2: 725–34, describes the exodus of merchants and others from trade and manufacturing as “the defection of the bourgeoisie.” Spanish entrepreneurial skills are discussed in García Sanz, *Segovia*, 216; Felipe Ruiz Martín, *Lettres marchandes échangées entre Florence et Medina del Campo* (Paris, 1965), xcix; Carmelo Viñas y Mey, “Notas sobre primeras materias, capitalismo industrial e inflación en Castilla durante el siglo XVI,” *Anuario de historia económica y social*, 3 (Madrid, 1970), 362–64.

<sup>42</sup> Antonio Domínguez Ortiz, *The Golden Age of Spain, 1516–1659*, James Casey, trans. (New York, 1971), 194–95; Braudel, *Mediterranean*, 2: 894.

<sup>43</sup> Pierre Chaunu, “Le Renversement de la tendance majeure des prix et des activités au XVII<sup>e</sup> siècle,” *Studi in onore di Amintore Fanfani* (Milan, 1962), 4: 219–55.

<sup>44</sup> Ruggiero Romano, “Tra XVI e XVII secolo: Una Crisi economica: 1619–22,” *Rivista storica italiana*, 74 (1962): 480–531; Romano, “Between the Sixteenth and Seventeenth Centuries.”

The volume of internal trade and the health of the commercial and financial fairs are more accurate than are bullion flows as indicators of the commercial life of Castile, but the fairs were subject to the government's unpredictable needs for money and the disruptions that epidemics, wars, and rebellions caused in the flow of trade. The Castilian fairs enjoyed their heyday in 1550–60 and thereafter faced increasing difficulties that coincided with the agricultural crisis and were related to it, though the relations have not yet been fully explored.<sup>45</sup> A bread shortage in 1583, particularly severe in Andalusia and New Castile, led to imports of Baltic grain. The hard currency needed to pay for the emergency imports reduced the coin in circulation at the Castilian fairs and may have created a shortage of cash and credit in the European financial network as a whole.<sup>46</sup>

Taxes for wars against rebels and foreign enemies in the late sixteenth century undoubtedly added to the strains on the internal economy, but taxes cannot be blamed for halting sixteenth-century growth. The crisis in the Castilian countryside was already apparent before tax increases surpassed inflation.<sup>47</sup> Taxes were much more damaging to the depressed economy that followed.

Scholars have sometimes called the seventeenth century a time of crisis, but that term is misleading. In fact, seventeenth-century Castile experienced a long, slow, and painful adjustment to the crisis of the late sixteenth century. Most notably, the population declined by nearly 20 percent, from a diversity of causes. First among them were several epidemics that struck in 1597–1602, 1647–52, and intermittently between 1677 and 1685.<sup>48</sup> In addition, some excess mortality may have occurred from famines and diseases related to chronic malnutrition. If so, it would provide evidence for Malthus's "positive check" in action. If not, it suggests that production, distribution, or adjustments in demographic behavior worked to avoid that ultimate curb to population growth. The current state of historical knowledge suggests that famines were rare in Castile, and that harvest shortages were ameliorated by the purchase of outside supplies, even at exorbitant cost. Historians

<sup>45</sup> Marcos Martín, *Medina del Campo*; William D. Phillips, Jr., and Carla Rahn Phillips, "The Castilian Fairs at Burgos, 1601–1604," *Journal of European Economic History*, 6 (Fall 1977): 413–29; Gerardo Moraleja Pinilla, *Historia de Medina de Campo* (Medina del Campo, 1971); Cristóbal Espejo and Julián Paz, *Las Antiguas ferias de Medina del Campo* (Valladolid, 1908).

<sup>46</sup> Ruiz Martín, *Letres*, xciii–xciv.

<sup>47</sup> Phillips, *Ciudad Real*, 81–94; Antonio Domínguez Ortiz, *Política y hacienda de Felipe IV* (Madrid, 1960); Ulloa, *Hacienda real*.

<sup>48</sup> Pérez Moreda, *Crisis*, 109, 290–306; Vicente Pérez Moreda, "The Intensity of Mortality Crises in Spain: An Outline of Their Regional Differences over Time," in *The Great Mortalities: Methodological Studies of Demographic Crises in the Past*, Hubert Charbonneau and André Larose, eds. (Liège, 1979), 179–98, 275. See also the other articles in the collection. Bartolomé Bennassar, *Recherches sur les grandes épidémies dans le nord de l'Espagne à la fin du XVI<sup>e</sup> siècle: Problèmes de documentation et de méthode* (Paris, 1969); Francis Brumont, "Le Coup de grâce: La Peste de 1599," in *Ciudad de Burgos*, 335–42; Domínguez Ortiz, *Sociedad española en el siglo XVII*, 1: 110–13; Antonio Domínguez Ortiz, *Córdoba: Apuntes para su historia* (Córdoba, 1981), 12; Bernard Vincent, "La Peste atlántica de 1596–1602," *Asclepio*, 28 (1976): 5–25; Bernard Vincent, "Les Pestes dans le royaume de Grenade aux XVI<sup>e</sup> et XVII<sup>e</sup> siècles," *Annales: économies, sociétés, civilisations*, 24 (November–December 1969): 1511–13; Luis Sánchez Granjel, "Las Epidemias de peste en España durante el siglo XVII," *Cuadernos de historia de la medicina española*, 3 (1964): 19–40; Mariano Peset and José Luis Peset, *Muerte en España (política y sociedad entre la peste y el cólera)* (Madrid, 1972); L. Carreras Panchón, "Las Epidemias de peste en España durante el siglo XVII," *Capítulos de la medicina española* (1964): 155–79. See also Carlo M. Cipolla, "The Plague and the Pre-Malthus Malthusians," *Journal of European Economic History*, 3 (Fall 1974): 277–84.

used to assume connections between harvest crises, fewer births, and excess deaths. Now they must prove them, and decades of work gathering data remain before reliable interpretations for Spain can be offered.<sup>49</sup> In 1677–85, for example, harvest failure, bad weather, monetary upheaval, and a combination of fatal diseases all struck at once.

Emigration and military recruitment also drained population from Castile in the sixteenth and seventeenth centuries, serving both as evidence of overpopulation and as part of its solution. The government's expulsion of the Moriscos in 1609–14 added to the outflow of people from Castile.<sup>50</sup> Although the incidence and effects of these losses varied in intensity depending on time and place, it is worth noting that some areas—for example, self-sufficient farming communities spared the waves of pestilence—may have emerged almost unscathed. Nonetheless, government officials and economic analysts (*arbitristas*) became concerned about depopulation even before 1600. They also observed with alarm the scarcity of marriages in many areas, a clear example of Malthus's "preventive check."<sup>51</sup>

Along with the population losses in seventeenth-century Castile and Andalusia came hints of a fundamental readjustment of people and resources. Agricultural production fell, commercial networks unraveled, and areas that had once relied on a broad region of supply found themselves thrown back on local resources.<sup>52</sup> The unraveling of commercial networks made the harvest crises of the seventeenth century particularly difficult for population centers to manage, but it was brutally efficient in reducing the size of towns and cities that were too large to be sustained from local resources alone. Population declines have been noted explicitly for the

<sup>49</sup> There is evidence for increasing poverty and population pressure in much of Europe at the end of the sixteenth century. Wilhelm Abel, *Agricultural Fluctuations in Europe from the Thirteenth to the Twentieth Centuries*, Olive Ordish, trans. (New York, 1980), 99–146; Emmanuel Le Roy Ladurie and Joséph Goy, *Tithe and Agrarian History from the Fourteenth to the Nineteenth Centuries*, Susan Burke, trans. (Cambridge, 1982), 93–119.

Vicente Pérez-Moreda and David S. Reher, "Demographic Mechanisms and Long-Term Swings in Population in Europe, 1200–1850," *International Union for the Scientific Study of Population, International Population Conference*, 4 vols. (Florence, 1985), 4: 313–29, argued that crisis mortality is the motor behind population change, with no clear link between living standards and mortality level. For a range of views, see *Hunger and History: The Impact of Changing Food Production and Consumption Patterns on Society*, Robert I. Rotberg and Theodore K. Rabb, eds. (Cambridge, 1985); Carl Mosk, "Nutrition and Fertility: A Review Essay," *Historical Methods*, 14 (Winter 1981): 43–46; Emmanuel Le Roy Ladurie, "A Concept: The Unification of the Globe by Disease (Fourteenth to Seventeenth Centuries)," in *The Mind and Method of the Historian*, Siân Reynolds and Ben Reynolds, trans. (Chicago, 1981), 50. Despite a wealth of scholarship, it remains difficult to separate the effects of privation and disease on population change.

<sup>50</sup> Henri Lapeyre, *Géographie de l'Espagne morisque* (Paris, 1959); Antonio Domínguez Ortiz and Bernard Vincent, *Historia de los moriscos: Vida y tragedia de una minoría* (Madrid, 1978), 199–206.

<sup>51</sup> Silva, *España*, 136; Gonzalo Anes Alvarez, *Las Crisis agrarias en la España moderna* (Madrid, 1970), 101–07. Molinié-Bertrand, *Siècle d'or*, 368–99, discusses contemporary economic treatises, including a lexicon of the words used most frequently to describe the state of the economy. See also Jean Vilar, *La Figura satírica del arbitrista en el Siglo de Oro*, Francisco Bustelo García del Real, trans. (Madrid, 1973); and Marjorie Grice-Hutchinson, *Early Economic Thought in Spain, 1177–1740* (London, 1978), 122–58.

<sup>52</sup> Gonzalo Anes Alvarez and Jean Paul Le Flem, "Las Crisis del siglo XVII: Producción agrícola, precios e ingresos en tierras de Segovia," *Moneda y crédito*, no. 93 (June 1965): 3–55; Jerónimo López-Salazar Pérez and Manuel Martín Galán, "La Producción cerealista en el arzobispado de Toledo, 1463–1699," *Cuadernos de historia moderna y contemporánea*, 2 (1981): 21–101; Enrique Llopis Agelán, "El Agro castellano en el siglo XVII: Depresión o 'reajustes y readaptaciones'?" *Revista de historia económica*, 4 (Winter 1986): 11–37; Maximiliano Barrio Gozalo, "Rentas de un grupo privilegiado del Antiguo Régimen: Los Arzobispos de Burgos, 1550–1835," in *Ciudad de Burgos*, 415.

areas around Córdoba, Segovia, Toledo, Jaén, and a number of other cities and their hinterlands.<sup>53</sup> Not every area suffered equally. The fortune of each small center of population around Segovia in the seventeenth century was linked to its specific mixture of subsistence farming, market farming, livestock rearing, and industry. A diversity of opportunities seems to have insulated some villages from acute distress, but there were others that prospered because they specialized in livestock, viticulture, or some other activity that paid well in a depressed economy.<sup>54</sup>

The fine cloth industry of Castile entered a deep depression in the seventeenth century, which affected major centers especially. Figures have not yet been developed for the overall performance of the textile industry, but the production of middling and coarse cloth seems to have survived, and even prospered, in small towns and in the rural putting-out industry. Large concentrated industries such as that in Segovia vanished, as did the political power of Segovian textile entrepreneurs. The owners of large flocks of sheep took their places in the governing body of the city; they were powerful enough by the middle of the century to bar merchants and clothmakers from official posts. It was a time of "deindustrialization and ruralization of the population."<sup>55</sup>

The production and export of raw wool also underwent a restructuring in the seventeenth century, although its precise lines are not yet clear. Transhumant flocks seem to have declined from about 1598 to 1625, but stationary flocks and those that traveled short distances probably increased.<sup>56</sup> Wool exports had fallen sharply with the agricultural crisis and European political troubles of the sixteenth century, but wool continued to find a market in Italy and Northern Europe. Castilian merchants, who had dominated the exports to Northern Europe in the sixteenth century, lost control of the trade, and no group of similar stature replaced them. Instead, Spaniards continued to buy wool from flockowners and see to its preparation for transport while small-scale merchants, mostly from the importing countries, fetched the wool at Spanish ports. In many ways, this new system paralleled what was happening in agriculture and industry. The elaborate network of supply and export that had been developed during the sixteenth century came undone in the seventeenth, leaving its component parts with only tenuous links to one another, and with no one group of merchants in control of the operation.<sup>57</sup>

Burgos, former center of the wool trade, entered a precipitous decline in the seventeenth century. Other cities were better able to carve out a new niche for

<sup>53</sup> Fortea, *Córdoba*, 471–75; David R. Ringrose, *Madrid and the Spanish Economy, 1560–1850* (Berkeley, Calif., 1983), 281–88; Marcos Martín, *Medina del Campo*, 252–53; Antonio Domínguez Ortiz, *Historia de Sevilla: IV: El Barroco y la ilustración* (Seville, 1976).

<sup>54</sup> García Sanz, *Segovia*, 58–74. See also María del Carmen Anson Calvo, *Aproximación a un estudio demográfico sobre la población leonesa: Siglos XVII y XVIII* (León, 1982).

<sup>55</sup> García Sanz, *Segovia*, 83, 218–21. See also Carrera Pujal, *Economía española*, 1: 417–20; Jean-Paul Le Flem, "Vraies et fausses splendeurs de l'industrie textile segovienne (vers 1460–vers 1650)," in *Produzione commercio e consumo dei panni di lana*, Marco Spallanzani, ed. (Florence, 1972).

<sup>56</sup> Le Flem, "Cuentas de la Mesta," 70–75, and tables; Llopis, "Crisis," 148–54.

<sup>57</sup> Phillips, "Wool Trade," 782–85, 793–95; Carla Rahn Phillips, "Spanish Merchants and the Wool Trade in the Sixteenth Century," *Sixteenth Century Journal*, 14 (1983): 259–82.

themselves, especially cities that had been regional market centers even before the boom of the sixteenth century. Distance from Madrid helped some cities avoid a loss of function, supplies, and population to the needs and opportunities of the capital. A range of middle-sized cities lost out to Madrid, whose rapid growth created a new network of trade that drew on virtually all of Castile.<sup>58</sup> The rise of Madrid was one of the most dramatic and long-lasting effects of the restructuring of trade in the seventeenth century.

Castilian trade with the New World also changed during the seventeenth century, with more silver remaining in the New World economy and more variety appearing in New World products sent to Europe. Foreign goods, shipping (including colonial ships), and merchants became increasingly prominent as well. The trade with Spain in metals and goods saw a depression for much of the seventeenth century, although reportedly widespread smuggling makes it difficult to gauge the change in the volume of trade.<sup>59</sup>

Governmental policies in the seventeenth century did little to aid the internal economy of Castile. Far-reaching reform plans were discussed frequently in the reigns of the last three Habsburgs, only to be abandoned as impractical.<sup>60</sup> Instead of reform, the government of Philip IV instituted new taxes and currency manipulations that were harmful to the beleaguered economy and slowed its recovery. It was not until the reign of the sickly and incompetent Charles II that a definite economic upturn took place.<sup>61</sup>

Population size edged slowly upward in the late seventeenth century, despite periodic setbacks. The epidemics of 1597–1602 and 1647–52 and the expulsion of the Moriscos may have fostered a demographic recovery by encouraging earlier marriages through increased economic opportunities for the remaining population.<sup>62</sup> In the absence of widespread birth control, an earlier age at marriage for women would have generated a higher birth rate and population growth. This growth, in turn, would have led to increased agricultural production and perhaps to higher productivity.<sup>63</sup> Local timetables varied, but there is increasing evidence

<sup>58</sup> Ringrose, *Madrid*, 143–63; Claude Larquie, "Etude de démographie madrilène: La Paroisse de San Ginés de 1650 à 1700," *Mélanges de la Casa de Velázquez* (1966): 225–58; Janine Fayard and Claude Larquie, "Hotels madrilènes et démographie urbaine au XVII<sup>e</sup> siècle," *Mélanges de la Casa de Velázquez* (1968): 229–58; María Carbajo Isla, "La Población de la villa de Madrid desde finales del siglo XVI hasta mediados del siglo XIX," *Boletín de la ADEH*, 2 (November 1984): 4–18.

<sup>59</sup> Chaunu, *Séville et l'Amérique*, 187–219, 247–67, 291–301; John C. TePaske and Herbert Klein, "The Seventeenth-Century Crisis in New Spain: Myth or Reality?," *Past and Present*, 90 (February 1981): 116–35; Michel Morineau, "La Crise du XVII<sup>e</sup> siècle," *Actes du VIII<sup>e</sup> Colloque de Marseille* (Marseille, 1978).

<sup>60</sup> Mario Lorenzo Sánchez, "The Attempts at Reform in the Spain of Charles II: A Revisionist View of the Decline of Castile, 1665–1700." (Ph.D. dissertation, University of Notre Dame, 1976); Pere Molas Ribalta, "La Restauración del Consulado de Burgos en el siglo XVIII," in *Ciudad de Burgos*, 430–32.

<sup>61</sup> See Domínguez Ortiz, *Crisis y decadencia*, 197–217, for a survey of the evidence and varying timetables postulated for the recovery. See also Gonzalo Anes Alvarez, *Economía e "ilustración" en la España del siglo XVIII* (Barcelona, 1969), 14; and Manuel Garzón Pareja, *La Industria sedera en España: El Arte de la seda de Granada* (Granada, 1972), 9–22.

<sup>62</sup> García Sanz, *Segovia*, 85, compares births and estimated harvests and finds a very favorable situation for population recovery in the seventeenth century.

<sup>63</sup> Grain production on five farms owned by the cathedral chapter of Córdoba suggests increased productivity as well as increased production. Ponsot, "Andalousie occidentale."



of a general demographic recovery in Castile by the 1680s, despite the epidemics and harvest failures of 1677–85.<sup>64</sup>

Large-scale manufacturing was much slower to revive, despite government attempts to encourage it, and Castilian products faced stiff competition from foreign goods on the international market.<sup>65</sup> Not enough data are available on cloth produced for local and regional markets, which must have accounted for the bulk of textile production.<sup>66</sup> It is possible that local cloth held its share of the market or grew with the recovery of the population. In Córdoba, where industry had long tended to be rural and dispersed, it became more so in the seventeenth century, eliminating much of the role that the city had played.<sup>67</sup> In Segovia, where the sixteenth-century textile industry may have been too centralized for the market, the seventeenth century marked a significant change. When textile production began to recover, Segovian textiles had become largely rural and decentralized. The continued slump in wool exports may have been related to the development of local textile production.<sup>68</sup>

Even though the start of economic revival predated any effects that historians can attribute to government, the governmental decrees and reform programs of the 1670s and 1680s at least aided the process. Most dramatic was the drastic devaluation of copper (*vellón*) coinage in 1680, in the midst of harvest failures and epidemics.<sup>69</sup> In the short term, the devaluation created much distress and dislocation in the internal economy and hurt exports by essentially doubling their price. In the long term, this action in 1680 and the devaluation of silver coinage in 1686 stabilized the currency and was mildly inflationary and stimulating to the economy. Other reforms of the bureaucracy and the tax structure also aided economic recovery, even though Spain was involved in several wars in the late seventeenth century. Although Castile continued to have a balance-of-payments deficit with its European trading partners, it was compensated by income from trade and taxes in the New World colonies.

The relationship between population and available resources governed the ebb and flow of the Castilian economy. Central to the demographic and economic behavior of the region as a whole were individual decisions made in response to changing local conditions. The combined effects of these decisions, in a favorable

<sup>64</sup> Vincent, "Récents travaux," 479; Pérez Moreda, *Crisis*, 325; Earl J. Hamilton, *War and Prices in Spain, 1650–1800* (1947; rpt. edn., New York, 1969), 135–36; Vilar, *Catalogue*, 1: 587; Marcos Martín, *Medina del Campo*, 258; J. M. Rabasco Valdés, "La Inmigración a Granada, 1665–1700: Fuentes para su estudio," *Actas*, 347–60; J. M. Rabasco Valdés, "Un Caso de aplicación de los registros parroquiales: Granada y la epidemia, 1640–1700," *Actas*, 299–304; Phillips, *Ciudad Real*, 30–34; Henry Kamen, "The Decline of Castile: The Last Crisis," *Economic History Review*, 2d ser., 17 (August 1964): 63–76. Henry Kamen, *Spain in the Later Seventeenth Century* (London, 1980), summarizes much of the available evidence, although the book lacks a clear sense of how the Spanish economy functioned.

<sup>65</sup> James Clayburn La Force, *The Development of the Spanish Textile Industry 1750–1800* (Berkeley and Los Angeles, 1965), 160–61.

<sup>66</sup> Agustín González Enciso, *Estado e industria en el siglo XVIII: La Fábrica de Guadalajara* (Madrid, 1980), 93–141.

<sup>67</sup> Fortea, *Córdoba*, 469–70.

<sup>68</sup> García Sanz, *Segovia*, 83–85; Phillips, "Wool Trade," 786–87.

<sup>69</sup> Hamilton, *War and Prices*, 35, 126–31; Lynch, *Spain under the Habsburgs*, 2: 267–80; Phillips, *Ciudad Real*, 81, 91–92; Kamen, "Decline of Castile."

ecological, legal, and social climate, allowed the population to expand notably in the late fifteenth and early sixteenth centuries. Even so, expansion ceased before encountering Malthus's "positive check" of mass starvation and diseases related to malnutrition. The mechanism involved is not yet clear. Decisions to emigrate played some role; perhaps decisions to delay marriage occurred as well. An epidemic in 1599 struck a population that had already ceased to grow. The net result was a situation favorable to another rise in population that continued through the eighteenth century. While avoiding the worst Malthusian corrective, Castile clearly experienced much short-term distress and the classic Malthusian cycle of boom and bust.<sup>70</sup>

OTHER PATTERNS WERE POSSIBLE WITHIN THE WORLD DEFINED BY Malthusian theory. In general, the northern and northwestern region had been more densely settled than Castile in the Middle Ages, although there was much variation at the local level. Like Castile, the north showed patterns of sixteenth-century population growth from moderate (50–70 percent) to high (70–90 percent).<sup>71</sup> (See Table 1.) In Galicia, where earlier development had been restricted by pirate raids and internal political instability, average population density rose from 12.8 per square kilometer in about 1530 to 21.8 in 1591—a stunning rise. Figures for the three Spanish Basque provinces of Vizcaya, Guipúzcoa, and Alava are less reliable, but population seems to have risen there as well. More interesting, however, the north and northwest may have escaped the drastic losses of population that afflicted inland Castile in the seventeenth century. The question is why, and evidence is still too fragmentary to do more than suggest the answers.

The north-northwest was a poor agricultural region, despite ample rainfall, or, more likely, because of it, a point that simplistic comparisons between wet and dry

<sup>70</sup> Francisco Bustelo García del Real, "Algunas reflexiones sobre la población española a principios del siglo XVIII," *Anales de economía*, 13 (1972). David R. Weir, "Life under Pressure: France and England, 1670–1870," *Journal of Economic History*, 44 (March 1984): 27–48, comments on the use of the Malthusian model in E. A. Wrigley and Roger Schofield, *The Population History of England, 1541–1871: A Reconstruction* (Cambridge, Mass., 1981). Traditional demographic historians have often viewed high birth and death rates as deliberately wasteful of life. Modern work tends to be much less judgmental, seeking to understand the ecological and cultural patterns that affected demographic decisions.

<sup>71</sup> Molinié-Bertrand, *Siecle d'or*, 76–129, 312; Emiliano Fernández de Pinedo, *Crecimiento económico y transformaciones sociales en el País Vasco, 1100–1850* (Madrid, 1974), 8–20; Luis María Bilbao and Emiliano Fernández de Pinedo, "La Evolución del producto agrícola bruto en la Llanada Alavesa, 1611–1813," *Actas*, 116–17; Rogelio Pérez Bustamante, *Historia de la villa de Castro Urdiales* (Santander, 1980), 43; José Luis Casado Soto, "La Vida en las villas portuarias," *Cantabria a través de la historia: La Crisis del siglo XVI* (Santander, 1979), 100–03; Agustín Rodríguez Fernández, "La Vida en el campo," *Cantabria a través de la historia: La Crisis del siglo XVI* (Santander, 1979), 53–62; Manuel Fernández Álvarez, Francisco Tuero Bertrand, and José Luis González Novalín, *Historia de Asturias*, vol. 6: *Edad Moderna I: Asturias en el siglo XVI* (Vitoria, 1977), 42–43; Baudilio Barreiro Mallón, "Interior y costa: Dos muestras de una estructura demográfica antigua en la Galicia rural," *Actas*, 387–412; Baudilio Barreiro Mallón, *La Jurisdicción de las Xallas en el siglo XVIII: Población, sociedad y economía* (Santiago de Compostela, 1977); Juan Eloy Gelabert González, *Santiago y la tierra de Santiago de 1500 a 1640* (Coruña, 1982); María Carmen González Muñoz, "Aproximación al estudio de la población gallega de 1579 a 1584: El Caso de la Ria de Ferrol," *Concepción arenal—ciencias y humanidades* (April 1984): 9–10. José Luis Casado Soto, "Aproximación al perfil demográfico de la villa de Santander entre los siglos XII y XVI," *Altamira*, 42 (1979–80): 51–65, discusses the sixteenth-century population crises in Santander, but his tables suggest that the population grew despite them.

Spain often overlook.<sup>72</sup> It was particularly difficult to grow wheat, the staple bread grain. Galicia relied on coarse rye bread, and, before the spread of maize and potato cultivation, the whole coastal area was frequently short of food.<sup>73</sup> Landholdings tended to be small and fragmented, and noble and ecclesiastical landowners controlled a much higher proportion of the land than in Castile, which limited the possibilities for agricultural expansion. On the positive side, iron deposits offered opportunities for industrial employment, and the presence of several fine, large seaports and numerous smaller ones meant that coastal inhabitants could make a living through fishing, shipbuilding, and commerce.<sup>74</sup> The combination of pressures and opportunities in each area determined the ways in which the inhabitants responded to rising population in the sixteenth century.

With limited opportunities to expand food production, preventive checks on population growth assumed a greater importance than they did in Castile. Evidence is growing that women married at a later age in the northern region than anywhere else, which would have reduced marital fertility and restricted population growth. The rate of permanent celibacy was also higher than in Castile.<sup>75</sup> Related to this behavior was a tradition of male emigration, especially notable in Galicia. Emigration, both permanent and temporary, was a conscious adaptation to chronically scarce resources, avoiding the danger of overpopulation. Galicians, who were less able than Castilians to tap the flow of wealth from the New World, and who were trapped in a situation of limited resources, developed a pattern of delayed marriage, male emigration, and a high rate of permanent celibacy. The situation was similar in the Basque provinces and for the same reasons. In addition, the government drew off experienced mariners and soldiers from the Cantabrian coast, adding to the traditional pattern of out-migration. Eventually, this loss of

<sup>72</sup> Kamen, *Spain*, 15–16, falls into this trap. “Wide variations in climate and geography created several economic units in the country rather than one. Differences in agricultural output between the parched interior and the richer coastlands led to serious deficiencies in the food supply.” Fernández de Pinedo, *País Vasco*, 35–50, more accurately analyzes the restricted options in the medieval Basque countryside. José Luis Casado Soto, *Cantabria vista por viajeros de los siglos XVI y XVII* (Santander, 1980), 146–47, includes a description of the Santander area in 1622 that mentioned the dense population, the scarcity of food supplies, and the start of maize cultivation. See also José Angel García de Cortazar, *Vizcaya en el siglo XV: Aspectos económicos y sociales* (Bilbao, 1966).

<sup>73</sup> Jaime García-Lombardero, *La Agricultura y el estancamiento económico de Galicia en la España del Antiguo Régimen* (Madrid, 1974), 43–45, contains dramatic graphics showing the scarcity of grain cultivation in Galicia.

<sup>74</sup> See Mariano Ciriquiain-Gaiztarro, *Los Puertos marítimos vascongados* (San Sebastián, 1951); the same author's *Monografía de la muy noble villa y puerto de Portugalete* (Bilbao, 1942), and the local histories cited in note 70 above, for traditional coastal employment.

<sup>75</sup> The earliest figures currently available for age at marriage and permanent celibacy come from the census of 1787, and the few reconstitution studies we have for earlier periods show the same patterns. The figures are conveniently displayed in Pérez Moreda, “Matrimonio,” 34–35. Given the glacial pace of change for demographic behavior, the same patterns probably held true for the entire early modern period. José Manuel Pérez García, “Demografía tradicional en dos localidades de la Galicia atlántica,” *Actas*, 437–62; José Manuel Pérez García, *Un Modelo de sociedad rural del Antiguo Régimen en la Galicia costera: La Península del Salnés (jurisdicción de la Lanzada)* (Santiago de Compostela, 1979). Although late marriage can be seen as proof of Malthus's preventive check in action (Joaquín Arango, “La Teoría de la transición demográfica y la experiencia histórica,” *Revista española de investigaciones sociológicas*, 10 [1980]: 176–77), it is unlikely that individuals decided to delay marriage in order to reduce their total marital fertility. Rather, they delayed marriage until they could afford to begin a household and family.

labor power became a hindrance to economic life on the coast, but in the sixteenth century it helped forestall a crisis as population rose. The pattern varied slightly, depending on the state of the economy, but it was determined by economic realities that transcended temporary fluctuations.<sup>76</sup>

Even while limiting population growth, the people of the north coast had a hard time providing themselves with a regular supply of food. Formidable mountains separated the Cantabrian coast from the Castilian plains, making overland transport both difficult and expensive. Port cities handling wool and other goods from Castile often compelled arriving caravans to bring grain as well. As Castilian grain became scarcer and more expensive in the late sixteenth century, a cheaper and more reliable supply was procured from abroad. The north coast was habitually exempted from the ceiling price on wheat and from prohibitions on the export of specie, when it was in exchange for grain.<sup>77</sup> Grain imports were also exempt from the embargoes on trade with enemy powers such as France in wartime. Modern historians refer to imports of grain to Castile, but these often mean imports of grain to the Cantabrian ports that served Castile.<sup>78</sup> How much of that grain was sent inland is not known, but interior Castile could not afford to import grain on a regular basis. Habitual imports were possible only for coastal areas, and then only if they had something to exchange or could obtain specie by trade with Castile. Because the north coast had access to foreign supplies of grain and purchased them regularly with governmental approval, the inhabitants had a further buffer against crisis. Even so, many areas entered a depression in the 1580s.

Given the weakness of agriculture, and even herding, on the north coast, it is natural that other occupations would have a prominent place in the economies of the area, yet scholars should guard against interpreting this diversity as a sign of economic advance. Iron production and fishing in Guipúzcoa and Vizcaya helped augment scarce resources in the sixteenth century, as shipping-for-hire had done in the late Middle Ages. Galicia was also heavily dependent on fishing. Following a period of expansion, the fishing industry suffered a crisis in the last third of the sixteenth century, probably caused by changes in the weather and overfishing. The crown's demands for sailors and ships also hurt the industry, which, by 1600, seems to have begun a decline that lasted for about a century.<sup>79</sup>

<sup>76</sup> Pablo Fernández Albaladejo, *La Crisis del Antiguo Régimen en Guipúzcoa, 1766–1833: Cambio económico e historia* (Madrid, 1975), 40. See also García-Lombardero, *Galicia*, 129–39; Hilario Rodríguez Ferreiro, “La Demografía de Hío durante el siglo XVIII,” *Actas*, 413–36; Hilario Rodríguez Ferreiro, *La Tierra de Trasdeza: Una Economía rural-antigua* (Santiago de Compostela, 1973); Antonio Meijide Pardo, *La Emigración gallega intrapeninsular en el siglo XVIII* (Madrid, 1960). A similar situation in neighboring northern Portugal led to the same pattern of late marriage, high celibacy, and emigration. Carolyn B. Brettell, “Nupcialidad en un pueblo de la provincia del Miño, 1700–1970: Una Nota de investigación,” *Boletín de la ADEH*, 2 (July 1984): 2–19; Maria Norberta Amorim, “Comportamentos demográficos do norte de Portugal durante o Antigo Regime,” *Boletín de la ADEH*, 2 (July 1984): 20–30.

<sup>77</sup> Fernández Albaladejo, *Guipúzcoa*, 46–47; Fernández de Pinedo, *País Vasco*, 21–23; Casado Soto, “Vida,” 103–05; Fernández Alvarez, *et al.*, *Historia de Asturias*, 6: 7–8.

<sup>78</sup> Braudel, *Mediterranean*, 1: 587, points out this misconception but fails to develop its implications.

<sup>79</sup> Fernández Albaladejo, *Guipúzcoa*, 40–41, 71–73. In the mid-eighteenth century, Galicia had 34 percent of the total registered fishermen in all Spain. R. Fernández Díaz and C. Martínez Shaw, “La

Iron production saw difficult times as well, probably related to trends in the European economy and Castile. High grain prices in Castile and the collapse of the northern wool trade at the end of the sixteenth century made contacts with Castile less profitable for the north coast and those with England and France more attractive. Guipúzcoa and Vizcaya fought for the relaxation of restrictions on foreign shipping, eventually winning the more-or-less free entry of foodstuffs in 1625.<sup>80</sup> But food had to be paid for, and the rising price of iron production, combined with a failure to make improvements in technology, made Basque iron less competitive. The first thirty years of the seventeenth century witnessed serious declines in iron production in both Vizcaya and Guipúzcoa, related in part to the European credit and industrial crisis around 1620. In the seventeenth century, the iron industry restructured, with the large foundries, most of which were in Vizcaya, surviving by introducing new techniques. Even so, only forged iron, preferred for fine arms and ship fittings, was truly competitive on the European market.

Guipúzcoa was less equipped to change, because its foundries tended to be smaller and undercapitalized. As the iron industry shrank, exports of crude iron ore increased, especially from Guipúzcoa. By 1640, iron founders in Vizcaya had persuaded the crown to tax those exports heavily to preserve more ore for its industry. Such an action might be seen as a positive move toward the protection of native industry, but its short-term effect was to plunge Guipúzcoa into ruin. Through most of the seventeenth century, the two provinces feuded with one another, each trying to gain governmental support and exclusive privileges. It was not until the end of the century, under the pressure of a French blockade, that they considered a consortium to trade their iron jointly for Castilian grain.<sup>81</sup>

A similar pattern of dependence on the European market and governmental support typified the shipbuilding industry. Although Basque shipbuilders could obtain wood, iron, and labor locally, they had to rely on the European market for naval stores, sails, and cable. Shipbuilding flourished from the fifteenth to the late sixteenth century, stimulated by the needs of commerce, empire, and warfare. By 1575, signs of decline appeared that may have been related to the increasing cost of supplies and the grain purchased from Castile. In other words, it was a crisis in the terms of trade much like the one faced by manufacturing areas in Castile as the price of food and raw materials rose. The situation worsened with the recession of the Indies trade in the 1620s, which diminished investment funds and the need for ships. Relying on foreign naval stores made the industry vulnerable to war in the Baltic and to the Dutch naval blockade during the Thirty Years' War.

Other problems included a shortage of skilled artisans and the difficulties of reducing construction costs. While the English and Dutch were producing lighter, cheaper ships that were ideal for commerce in peaceful waters, the Cantabrian

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Pesca en la España del siglo XVIII: Una Aproximación cuantitativa (1758–1765),” *Revista de historia económica*, 2 (Autumn 1984): 183–202.

<sup>80</sup> Fernández Albaladejo, *Guipúzcoa*, 46–47.

<sup>81</sup> Fernández Albaladejo, *Guipúzcoa*, 58–64; Fernández de Pinedo, *País Vasco*, 30–33.

shipbuilders continued to turn out the heavy ships required for Spanish trade and imperial defense in hostile waters. Because of its military importance, the shipbuilding industry received considerable attention from the government, including subsidies and continued contracts for new shipping. In fact, in the early seventeenth century, the crown had ships built on commission more often, rather than relying heavily on embargoes of private merchant vessels in wartime.<sup>82</sup> These government commissions did not compensate for the declines in private construction, although they did manage to prevent the total ruin of shipbuilding on the north coast.

Commerce from the Cantabrian coast was the lifeline of the port towns and the region as a whole, as it had been for centuries. At the height of the wool trade in the mid-sixteenth century, a great volume of wool flowed out of, and foreign goods flowed into, the northern ports. The trade was largely in the hands of merchants from Old Castile, especially the members of the merchant council of Burgos. The *Consulado* had legal rights over the functioning of many Cantabrian ports, which in some ways can be considered outposts of inland Castile rather than independent towns. With the decline of the wool trade from the 1570s on, those ports that handled Castilian wool—primarily Bilbao, Laredo, and Santander—entered a steep decline. But San Sebastián, which usually handled the coarser wools of Navarre, saw its fortunes rise in the first third of the seventeenth century, as coarser wools found an improved market in the textile industries of Northern Europe.<sup>83</sup> In exchange, San Sebastián could purchase much-needed foreign grain, and the privilege of being able to export specie to pay for it made San Sebastián a conduit through which Northern Europe could tap the wealth coming through Seville. San Sebastián benefited from the reorganized shipping networks in the seventeenth century, one of the few ports on the north coast to do so and then only until trade shifted to Bayonne after 1659.<sup>84</sup> Overall, the seventeenth century saw the definitive shift of Spanish commerce to the ports of Andalusia, especially Seville and Cádiz, with their access to the New World.

Economic recovery arrived on the north coast in the late seventeenth century, based on a restructuring of commerce and industry. After about 1650, the wool trade to Northern Europe enjoyed a modest revival, from which the port towns could benefit without the interference of Burgos. Native merchants in Bilbao became active participants in the trade, serving as intermediaries between foreign buyers and internal merchants and flockowners. Although north-coast merchants cannot be said to have replaced the Burgalese in power and function, they carved

<sup>82</sup> Carla Rahn Phillips, *Six Galleons for the King of Spain: Imperial Defense in the Early Seventeenth Century* (Baltimore, Md., 1986), 19–33.

<sup>83</sup> Phillips, "Wool Trade," 786–87; Fernández Albaladejo, *Guipúzcoa*, 76–77; Jacques Heers, "Commerce des Basques en Méditerranée au XV<sup>e</sup> siècle (d'après les archives de Gênes)," *Bulletin hispanique*, 57 (1955): 292–323; Mercedes Mauleón Isla, *La Población de Bilbao en el siglo XVII* (Valladolid, 1961).

<sup>84</sup> Fernández Albaladejo, *Guipúzcoa*, 78–82; Fernández de Pinedo, *País Vasco*, 19; Robert Sidney Smith, *The Spanish Guild Merchant: A History of the Consulado, 1250–1700* (Durham, N.C., 1940), 81; Jaime de Estánislao Labayru y Goicoechea, *Historia general del Señorío de Bizcaya*, vols. 3–5 (Bilbao, 1899–1901), summarized as *Compendio de la historia de Bizcaya* (1873; rpt. edn., Bilbao, 1975), 276.



out a comfortable niche for themselves in the decentralized wool trade of the late seventeenth century.<sup>85</sup>

In agriculture, new crops and methods fundamentally altered the economic base of the region. In the long term, the dense population and scarce resources of northern Spain encouraged innovations, whereas the low population density and available land of Castile did not.<sup>86</sup> The use of lime was perhaps the greatest improvement, leading to better drainage of clay soils and allowing for the more rapid assimilation of nitrates and other nutrients. The cultivation of maize, mostly for animal fodder, led to better aeration of the soil, and the introduction of the colewort plant produced both fodder and rapeseed oil. With new sources of fodder, livestock density could be increased, leading to the production of more manure, as well as other animal products. The end result was more efficient use of the land and higher agrarian incomes in many parts of the north coast in the late seventeenth century. In Guipúzcoa, for example, the first phase of expansion (from about 1650 to 1700) took place on land already used for agriculture, so that it posed no threat to pasture or forest. Population grew, and, despite setbacks, commerce and industry revived along with changes in agriculture. Renewed population pressure developed in the mid-eighteenth century, but by then the resource base had greatly expanded, compared to earlier times.<sup>87</sup>

The north coast fit within the same Malthusian world that enclosed Castile, with certain differences. More limited agricultural resources in the north led to a stronger tradition of late marriage, celibacy, and emigration than in Castile, despite short-term changes in the economy. Diverse employment opportunities and numerous seaports provided the wherewithal and the opportunity for the area to be supplied in part by foreign grain. The government, by supporting shipbuilding, permitting the export of specie in exchange for grain imports, and (paradoxically) by drawing off mariners for official duty, helped the north coast to balance population with available resources more effectively than Castile could do. Whereas changing economic conditions in Castile could lead to wide fluctuations in population size, the north was buffered by customs that had evolved to cope with habitual scarcity.

IN THE EASTERN PROVINCES OF THE Crown of Aragón—Aragón, Catalonia, and Valencia—and in Murcia, similar positive and negative incentives combined to avoid a population crisis. Historians of the Crown of Aragón have often

<sup>85</sup> Phillips, "Wool Trade," 786–87; Fernández de Pinedo, *País Vasco*, 58–59.

<sup>86</sup> See Boserup, *Conditions of Agricultural Growth*, for the best discussion of this mechanism of agrarian change.

<sup>87</sup> Fernández Albaladejo, *Guipúzcoa*, 85–99; Barreiro, "Producción agrícola," 99–101; Fernández de Pinedo, *País Vasco*, 24–28; Antonio Eiras Roël, "Evolución del producto decimal en Galicia a finales del Antiguo Régimen; Primeras series diezmales," *Actas*, 77–82; María Xosé Rodríguez Galdo and Xosé Cordero Torrón, "Rentistas urbanos y capital usurario: La Aparcería de ganado en Galicia en el siglo XVIII," *Revista de historia económica*, 2 (Autumn 1984): 287–94; Smith, *Guild Merchant*, 90; Casado Soto, *Cantabria vista*, 146–47; and Fernández Álvarez, *et al.*, *Historia de Asturias*, 6: 42–44, 69. Maize may have had an early impact in some areas. The data are still too sparse for certainty, but Asturias may have escaped the seventeenth-century depression altogether. Gonzalo Anes, *Historia de Asturias*: vol. 7: *Edad Moderna II: El Antiguo Régimen, economía y sociedad* (Vitoria, 1980), 5–18, 28–30.

emphasized its differences from the rest of Spain, particularly from Castile.<sup>88</sup> Although separated by topography from the sources of Castilian grain, the eastern area could easily be supplied by sea. In bad times, especially, the larger international market could have an ameliorating effect on both the price and the availability of grains. But, beyond that, the eastern provinces also embraced areas that habitually produced grain surpluses, along with areas that habitually faced shortages. The Crown of Aragón had a tradition of administrative independence from Castile that was zealously defended, though not always with success. One of the most important aspects of this independence was a coinage separate from the wildly fluctuating seventeenth-century currency of Castile. Trade between the eastern region and Castile was, nonetheless, important to both areas. The difference in their experience was not the result of a lack of contact between them but of differences in their geography, resources, and traditions.

Contrary to the traditional view that Catalonia experienced economic decay from the fourteenth to the early sixteenth century, more recent evidence shows a substantial economic recovery during the fifteenth century, similar to that of Castile. Catalonia underwent the same demographic growth in the sixteenth century evident elsewhere, although it was slower than in central Castile. Even before the rise, areas of high population density existed in the eastern region, although with great variations among residents of the mountains, the plains, and the coasts. Banditry, a practice often linked to high population density, characterized some areas. The port of Barcelona did not begin to experience the growth of many Castilian cities in the sixteenth century. The population of Barcelona rose from under 30,000 around the year 1500 to 46,000 by 1552, and perhaps to a somewhat higher peak before it fell back to below 40,000 by the end of the century.<sup>89</sup> One reason for this fluctuation was the shift in trade away from the Mediterranean and toward the Atlantic. Although individual Catalan merchants sought to expand their business in Castile and the Andalusian gateway to the New World, their efforts did not compensate for losses in the city's trade with other Mediterranean ports. When the Burgos–Bilbao–Flanders axis weakened from the late 1560s onward, the Barcelona–Genoa trade route revived, but it was dominated by Genoese, and Barcelona itself became part of the trading orbit of Marseilles.<sup>90</sup>

Along with declining trade, historians have usually blamed overcrowding and epidemics for the late sixteenth-century fall in Barcelona's population, which

<sup>88</sup> Pierre Vilar, *La Catalogne dans l'Espagne moderne*, 3 vols. (Paris, 1962), represents this view strongly, seeing links with Castile as the source of harm to Catalonia, and equating prosperity with Catalonia's ability to stay clear of Castile's influence.

<sup>89</sup> Vilar, *Catalogne*, 1: 461–64, exemplifies the traditional view. See also Nadal, *Población*, 71, and Josep Iglésies, *Estadísticas de población de Catalunya el primer vicenni del segle XVIII*, 2 vols. (Barcelona, 1974), and the same author's "El Poblament de Catalunya durant els segles XIV i XV," *Congreso de Historia de la Corona de Aragón, celebrado en Cerdeña en los días 8 al 14 de diciembre del año 1957* (Madrid, 1959), 6: 247–70; Ricardo García Carcel and M. V. Martínez Ruiz, *Población, jurisdicción y propiedad del Obispado de Gerona, siglos XIV–XVII* (Gerona, 1976); José M. Martínez Carrión and Miguel Rodríguez Llopis, *Las Transformaciones demográficas de la población rural: Yeste en los siglos XIV al XX* (Murcia, 1983); and Pérez Moreda, "Intensity," 179–86. Vincent, "Récents travaux," 468, reports on other published work.

<sup>90</sup> Vilar, *Catalogne*, 1: 539–52, 584–85, 617–20.

would agree with what we know about the disease patterns of major Mediterranean ports. Overall, however, the population in the eastern region may have continued to grow until about 1610–20.<sup>91</sup> Immigration from France was an important feature of this growth, and it was especially strong toward Catalonia in 1580–1600, just when the population rise in Castile faltered. It has been estimated that French immigrants made up 20 percent of the population of Catalonia in the late sixteenth century. The French may have accounted for 20 to 25 percent of the population of Aragón and a much smaller percentage of the population of Valencia. By 1626, it was estimated that there were 200,000 French in Spain, most of them in the eastern provinces.<sup>92</sup> Yet, despite moderate population growth and impressive immigration, the eastern kingdoms still comprised only about 14 percent of the total Spanish population at the end of the sixteenth century (see Table 2).

French immigration and the continued growth of the population into the seventeenth century suggest, among other possibilities, that the eastern provinces of Spain were better off than either Castile or the southern provinces of France. But, since French immigrants traditionally came from poor and overpopulated regions such as the Haute Auvergne and Languedoc, the timing may suggest that those areas were approaching the same Malthusian crisis that afflicted Castile. In some recent work on eastern Spain, hints of a similar crisis appear as well, masked, ironically, by the flow of immigrants.<sup>93</sup> For the countryside of Murcia on the southeast coast, legally a part of Castile, there is evidence of difficulty in agriculture by the late sixteenth century. The situation was similar in adjacent Alicante and in Valencia from the mid-1570s. The city of Murcia, which had experienced moderate growth in the sixteenth century, held up better, well supplied with food from the market gardens surrounding it and from imported supplies of wheat.<sup>94</sup>

Aragón, despite its poverty, normally produced a surplus that was sold to Catalonia and Valencia, and sometimes even to Castile, Navarre, and France. Local

<sup>91</sup> Valentín Vázquez de Prada, *Historia económica y social de España*, vol. 3: *Los Siglos XVI y XVII* (Madrid, 1978), 119, 123; Josep M. Salrach and Eulalia Durán, *Historia dels països catalans: Dels orígens a 1714*, 2 vols. (Barcelona, 1981), 2: 1058–61; Gregorio Colás Latorre, *La Bailia de Caspe en los siglos XVI y XVII* (Zaragoza, 1978), 15–19; Gregorio Colás Latorre and J. A. Salas Ausens, *Aragón bajo los Austrias* (Zaragoza, 1977); Gregorio Colás Latorre and J. A. Salas Ausens, *Aragón en el siglo XVI: Alteraciones sociales y conflictos políticos* (Zaragoza, 1982); V. Bielza de Ory, *La Población aragonesa y su problemática actual*, 2d edn. (Zaragoza, 1979), 15–17.

<sup>92</sup> Nadal and Giralt, *Population catalane*, 80–81; Nadal, *Población española*, 77–78; María del Carmen Ansón Calvo, "Zaragoza como lugar de inmigración en el siglo XVII," X. *Congreso de Historia de la Corona de Aragón* (Zaragoza, n.d.), 27–30; Abel Poitrineau, "La Immigration française en el reino de Valencia (siglos XVI–XIX)," *Moneda y crédito*, 137 (June 1976): 103–33; James Casey, *The Kingdom of Valencia in the Seventeenth Century* (Cambridge, 1978), 6–7.

<sup>93</sup> See the figures in Colás Latorre, *Bailia de Caspe*, 37–58; Casey, *Valencia*, 74–76; Carmen María Cremades Griñán, *Alimentación y consumo en la ciudad de Murcia durante el siglo XVIII (1701–1766)* (Murcia, 1984); Primitivo J. Plá Alberola, "La Natalidad ilegítima en el condado de Cocentaina (siglos XVI al XIX)," *Anales de la universidad de Alicante: Historia moderna* (1983): 21–23; Primitivo J. Plá Alberola, *La Población del Marquesado de Guadalest en el siglo XVII* (Alicante, 1983); Onofre Vaquer Bennassar, "La Nupcialidad en Felanitx (Mallorca) en los siglos XVI y XVII," *Boletín de la ADEH*, 2 (July 1984): 31–38; and Pedro Pérez Puchal, "Estado actual de las investigaciones sobre población valenciana," *Estudios geográficos*, 36 (August–November 1975): 951–73; Henri Lapeyre, *La Taula de Cambis (en la vida económica de Valencia a mediados del reinado de Felipe II)* (Valencia, 1982), 115–25.

<sup>94</sup> Francisco Chacón Jiménez, *Murcia en la centuria del quinientos* (Murcia, 1979), 117–125; María Teresa Pérez Picazo and Guy Lecomier, "Nota sobre la evolución de la población murciana a través de los censos nacionales (1530–1970)," *Cuadernos de investigación histórica*, 6 (1982): 5–10.

authorities in Aragón alternated between allowing and prohibiting the free export of grain. Western Catalonia also normally produced a surplus, but high transport costs and a poor distribution network made the surplus difficult to export. Barcelona's grain supplies usually came from coastal cities such as Tarragona, and from Aragón, but grain from as far away as Sicily was sometimes cheaper and easier to procure. A free market in grain from 1580 to 1600 brought a surplus to the city, which could explain why Barcelona continued to attract foreign immigrants after 1600. A ceiling price set in 1599 drove some local grain out in search of higher prices, and scarcity and bread riots in 1604–06 led to imports coming from as far away as England. For the most part, however, a free market in grain characterized Catalonia, except during serious bread crises.<sup>95</sup>

Similarly in Valencia, a habitual importer of grain, the population supplemented the harvests with free market supplies, most of them from foreign sources, because Castilian exports were often restricted. The people of Valencia paid for the grain with money earned selling silk. The production of Valencian raw silk peaked around 1580, and the silk cloth industry likewise rose until the 1570s, benefiting from the economic boom in Castile, the major client for Valencian silk cloth. By the late sixteenth century, Valencia was buying grain from Northern Europe, and it continued to do so.<sup>96</sup> The timing and volume of grain imports can be indicative of harvest crises, but historians should be careful not to assume that grain imports in general are a sign of economic weakness. Assuming that imports can be paid for, they also suggest economic diversity and wealth. They indicate the range of options available to the eastern coastal regions of Spain.

By the same token, the east coast and its hinterlands were particularly vulnerable to imported diseases, which often came to Spain from the Mediterranean and southern France.<sup>97</sup> Epidemics, a fairly high population density in good agricultural regions, and the poverty of much of the province of Aragón may have slowed population growth in the sixteenth century, reducing the risk of serious overpopulation. Like the north, the eastern provinces may have been buffered against crisis, not by their wealth but by the limits on their resources and the health of their populations.

The generally favorable conditions in the eastern provinces turned downward by the first third of the seventeenth century, marked by population stagnation in some places and decline in others. The expulsion of the Moriscos in 1609 had serious short-term consequences in Aragón and in Valencia, where the government expelled over 30,000 families in 1609, roughly one-third of the population. The remaining population was afflicted by the epidemics that arrived on the Levantine coast during the seventeenth century, hindering the effects of resettlement in the Valencian countryside. Agriculture was further harmed by bad weather and poor harvests in the early and middle seventeenth century. The flow

<sup>95</sup> Salrach and Durán, *Catalonia*, 2: 1059–78; Vilar, *Catalogne*, 1: 555–58, 599–602.

<sup>96</sup> Casey, *Valencia*, 29–31, 60–62, 79, 98–99.

<sup>97</sup> Vilar, *Catalogne*, 1: 525–28, discusses the sixteenth-century epidemics in Catalonia. See also Jorge Nadal and Emilio Giral, "Ensayo metodológico para el estudio de la población catalana de 1552 a 1717," *Estudios de historia moderna*, 3 (1953): 245–46.

of immigrants dried up from about 1620 on, coinciding with a general downturn in the economy related to diminished trade with Castile and monetary disorders.<sup>98</sup>

More than in Castile, however, the economic downturn in the eastern region was linked to broader European commercial trends. By 1620–30, Catalan trade with Italy amounted to one-third of what it had been as recently as 1600, partly because of declining exports of cloth and iron. Foreigners in Catalonia exported raw iron ore and wool, which (so it was charged) ruined local industries. A memorandum of 1630 claimed that the number of wool weaving guilds had fallen by two-thirds in thirty years. It was the same for silk and leather. A more likely reason for the decline of the textile industry than the export of raw materials was the import of foreign cloth, especially from France. Cheap and attractive foreign cloth flooded the market, partly as a consequence of the overvaluation of gold in Catalonia. Some attempts were made to protect Catalan industry, but the bulk of official opinion favored a free market and fought guild pressure to restrict imports. A free market precipitated the decline of local industry, temporarily at least. Reaching a peak in 1579, silk cloth manufacturing in Valencia entered a decline that lasted until after the mid-seventeenth century. The period following the expulsion of the Moriscos was especially hard, as the loss of workers in the countryside reduced harvests and rents, depressing even further the demand for manufactured goods. In steel-making, Catalonia lost out definitively to Northern Europe by the 1620s, and, in the Peace of the Pyrenees (1659), Catalonia lost the best local sources of iron to France.<sup>99</sup> The underlying difficulty seems to have been an inability to keep up with technical advances, just as in the Basque iron industry, although the Basques continued to control their raw materials.

The Catalan rebellion against the crown in 1640–52 marked an economic slump for that province, bringing with it monetary alterations worse than those of Castile, a pervasive French influence in the economy, and, eventually, a devastating plague that affected other parts of Spain and hastened the collapse of the rebellion.<sup>100</sup> After the Peace of the Pyrenees allowed French cloth into Catalonia without restriction, it flooded the market, despite local pleas for protection. Revaluations of Catalan money in 1660–74, after the coinage was stabilized in 1654, nonetheless signaled that the economy was recovering along with the population.<sup>101</sup> Monetary stability encouraged trade of all kinds. Signs of increasing port traffic in Barcelona between 1664–74 and 1679–80 are evident; by the end of the century, the level of port traffic was double what it had been in 1664–74. Grain prices in Barcelona fell in the late 1680s because of imports, and the prices of locally produced wine

<sup>98</sup> Casey, *Valencia*, 74–76; Colás Latorre, *Bailía de Caspe*, 51–56; Juan Ramón Torres Morera, *Repoblación del reino de Valencia después de la expulsión de los moriscos* (Valencia, 1969); Pérez Picazo and Lemeunier, “Población murciana,” 10–11.

<sup>99</sup> Vázquez del Prada, *Historia económica*, 559–60; Vilar, *Catalogne*, 1: 602–17; Casey, *Valencia*, 65–75, 87–90.

<sup>100</sup> Vilar, *Catalogne*, 1: 633; Casey, *Valencia*, 10–14; Jesus Maiso González, *La Peste aragonesa de 1648 a 1654* (Zaragoza, 1982).

<sup>101</sup> Vilar, *Catalogne*, 1: 625–35; *Companya nova de Gibraltar: Le “Manual de la Companya nova” de Gibraltar, 1709–1723*, Pierre Vilar, ed. (Paris, 1967), 81. A. Segura and J. Suau, “La Demografía histórica de Mallorca,” *Boletín de la ADEH*, 4 (March 1986): 64–71, show a population rise in Mallorca as well from 1667 on.

and oil rose, which benefited areas such as Reus and Mataró. Catalan wine was also exported to Castile, especially to Cádiz for the Indies trade.<sup>102</sup> Adequate harvests in 1660–80 marked the start of agricultural recovery in Valencia, which paralleled the recovery of Castile and much of Western Europe as a whole.<sup>103</sup> The province of Murcia, less well supplied by sea than were other parts of the eastern seaboard in the late seventeenth century, experienced volatile fluctuations in grain prices similar to those that occurred in the interior of Castile.<sup>104</sup>

It is not clear whether the slump of 1620–70 caused a restructuring of industry in Catalonia, but there are hints that it did. When the textile industry began to recover in about 1680, it often did so in places outside guild regulations and through the use of foreign styles and methods. The revival was fostered by businesspeople and rich artisans, and it was supported by generally favorable economic conditions, monetary stability, and the interest of the government in reviving and protecting industry. Even Catalan shipbuilding, which had been in decay since the fifteenth century, enjoyed a modest revival from 1675–80 on, mostly in the smaller shipyards that built small vessels for private owners rather than military vessels for the crown.<sup>105</sup> For those who could benefit from the revival, the reign of Charles II was a prosperous time. The author of *Anales de Catalunya* (1709) dubbed him “the best king that Spain has ever had,” without, it seems, even a hint of irony.<sup>106</sup>

In Valencia, the silk industry began to revive in the 1670s, with an expansion of mulberry production and an improved market in Castile.<sup>107</sup> The evidence to date suggests that a considerable restructuring of the industry took place in the seventeenth century and was marked by an increased use of rural workers. Silk production suffered from undercapitalization and from too heavy a dependence on the Castilian market, but its revival is clear. Valencia’s overall economic timetable was strikingly similar to Castile’s, with an expansion that ended about 1580, then stagnation, decline, and unmistakable signs of revival by 1680. The significant differences between Valencia and Castile were the greater impact of the Morisco expulsion on Valencia and its ability to import grain regularly. Grain imports, however, were only possible as long as Valencia earned money by selling its silks to Castile; thus Valencia was closely tied to the Castilian economy. Elsewhere in the east, the timetable showed greater divergence from Castile. Nonetheless, recent evidence reveals more similarities than were previously apparent and encourages the formulation of a model to encompass the Spanish economy as a whole.

<sup>102</sup> Vilar, *Catalogne*, 1: 638–48; Vilar, *Companya*, 83; Luis J. Navarro Miralles, “Datos para un estudio urbano—y riqueza—en el corregimiento de Tarragona en la primera mitad del siglo XVIII,” *Revista de historia económica*, 2 (Autumn 1984): 245–62.

<sup>103</sup> Casey, *Valencia*, 74–76; Le Roy Ladurie and Goy, *Tithe*, 131–53.

<sup>104</sup> C. Caro López, “Las Oscilaciones del precio del trigo en una ciudad del Levante: El Caso de Murcia, 1675–1800,” *Revista de historia económica*, 3 (Spring–Summer 1985): 259–60.

<sup>105</sup> Vilar, *Catalogne*, 1: 595–98, 649–70; Vilar, *Companya*, 83–84.

<sup>106</sup> Narcís Feliu de la Penya, *Anales de Cataluña*, 3 vols. (Barcelona, 1709), quoted in Vilar, *Catalogne*, 1: 671.

<sup>107</sup> Casey, *Valencia*, 84–86.



JUDGING FROM THE EVIDENCE CURRENTLY AVAILABLE, the model that best explains the economy of early modern Spain is a Malthusian one, centering on the relationship between population and available resources. Both sides of the relationship must be defined in sufficient detail to allow for a diversity of ecological, social, and legal realities. The northern and eastern peripheries of Spain were more densely settled than was Castile, in the areas where people settled at all. Given the dense settlements, the structures of landownership, and the local resource base, the populations of the peripheries had little room for the expansion of cultivation. But these factors did not make them more vulnerable than Castile to Malthusian pressures. Instead, I would argue that they were less so, because the scarcity of resources discouraged early marriage and instilled a tradition of high celibacy and emigration. In addition, in both the north and east, external commerce lessened the effects of harvest fluctuations while it drew the local populace into the broader orbits of European epidemics and economic trends.

By contrast, in Castile, a low population density, the availability of land, and direct connections with the markets of the New World could stimulate a rapid but precarious expansion of agriculture under favorable conditions, as happened during the sixteenth century. The Castilian interior could rarely overcome food shortages through long-distance imports and was also more vulnerable to the pernicious effects of tax increases and inflation. By the late sixteenth century, the tension that held population and resources in approximate balance was disturbed, exposing the inflated population to the full force of harvest failures and disease.

Because of Castilian dominance in both the politics and economy of Spain, historians have, understandably, come to think of the decline of Spain as belonging primarily to Castile, and have linked economic decay in the seventeenth century with governmental policies and Spain's faltering power in European politics. Scholars of the northern and eastern regions have been well disposed to accept this interpretation because it sets their regions apart from the centralizing monarchy of Castile, which has often been viewed as an unwelcome intrusion in the periphery. I have argued, however, that the regional components of the Spanish economy need to be analyzed together as well as separately, within the same model. Malthus defined the first version of such a model in the late eighteenth century, ironically, just as it was losing its explanatory power for parts of Western Europe. The model nevertheless very much applies to the sixteenth and seventeenth centuries. Even allowing for regional variations, a Malthusian model fits the Spanish economic experience. Moreover, the Spanish experience has more in common with broader European developments than historians have previously acknowledged.

Regarding demographic behavior, the model predicts that the resources available to sustain life will be inversely related to the age at marriage, the rate of permanent celibacy, and the propensity to emigrate. These resources include the ratio of land to labor and the diversity of possibilities for making a living. Within the context of ecological, social, and legal norms, demographic decisions will define regional behavior. Because of the great diversity of the Spanish landscape, a similar diversity in local demographic patterns is to be expected. Even so, regional

comparisons can provide a useful framework for investigation and prove particularly helpful in supplying the context currently lacking in much of the new demographic work being published about Spain.

The implications of the Malthusian model for the relationship between politics and the economy are less clear. Studies of European population and agricultural production in the early modern period now cover a wide range of countries, vastly different in total population, governmental economic policies, and relative positions in the European hierarchy of power. Yet nearly every study shows an economic and demographic pattern similar to the one analyzed here for Spain, especially with regard to sixteenth-century growth. The late sixteenth and the seventeenth century show more variety, with crises of different magnitude and duration.<sup>108</sup> There are hints that a similar pattern could pertain to the whole Northern Hemisphere.<sup>109</sup> If true, they would provide further evidence that historians must look to global weather patterns to find a possible source for the long-term swings in population and agriculture.

By the late seventeenth century in some areas of Europe, land quality, population density, and opportunities for market specialization favored intensive cultivation. In those areas, people were able to break free from the Malthusian cycle of boom and bust, expansion and retrenchment. Parts of England and the Netherlands provide notable examples of this. Yet, neither the escape from Malthus nor the preceding cycles of rising and falling economic indicators originated with governmental policies. Instead, such policies reacted to economic conditions and attempted to shape them. As Leopold von Ranke observed, "A sovereign can only promote, he cannot create; he may impede, but never can he singly destroy."<sup>110</sup> As historians, we should acknowledge connections between government and economic development where evidence shows they exist; we cannot simply assume the connections. And we should remember that economic power was not necessarily associated with political power. The Dutch enjoyed their brief golden age of commercial dominance with no pretensions to political hegemony; the Spanish enjoyed more than a century of political hegemony and four centuries of empire, despite meager economic resources. To understand the changing fortunes of Spain in the early modern period, we need to understand both political and economic developments, and we must explore the connections between them with an open mind.

<sup>108</sup> The findings of hundreds of local and regional studies are summarized in Abel, *Agricultural Fluctuations*, 92–193; and Le Roy Ladurie and Goy, *Tithe*, 81–153. On the whole, the seventeenth-century timetable is better presented by Le Roy Ladurie and Goy, who rely on tithe figures as indications of production. Abel relies more heavily on price series, which are affected by the availability of money and market forces, among other things, and are thus less accurate as indications of production. See Thomas J. Scheaper, *The Economy of France in the Second Half of the Reign of Louis XIV* (Montreal, 1980), for local studies challenging the idea that the French economy plummeted in the late seventeenth century.

<sup>109</sup> Woodrow Borah, *New Spain's Century of Depression* (Berkeley, Calif., 1951), 1–2, 22–28, 34, 43–44, reported a timetable for Mexico which I find strikingly similar to that for Castile. See also Ping-ti Ho, *Studies on the Population of China, 1368–1953* (Cambridge, Mass., 1959), 257–70; Marcel Reinhard, André Armengaud, and Jacques Dupâquier, *Histoire générale de la population mondiale*, 3d edn. rev. (Paris, 1968), 108–73.

<sup>110</sup> Leopold von Ranke, *The Ottoman and the Spanish Empires in the Sixteenth and Seventeenth Centuries* (New York, 1975, from the 1843 London edn.), 98–99. See Vilar, *Catalogne*, 1: 510, for a similar observation.

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# There Was No “Absalom” on the Ball Plantations: Slave-Naming Practices in the South Carolina Low Country, 1720–1865

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CHERYLL ANN CODY

FOR HISTORIANS OF THE BLACK EXPERIENCE UNDER SLAVERY, the process by which Africans became Afro-Americans presents an intriguing set of questions. Many students of this transition have turned to some version of an “encounter model,” to borrow the term anthropologists use, to identify and weigh the relative contributions of African and white American cultures to black societies of the New World.<sup>1</sup> Historians have long recognized that white “American” culture did not entirely subsume African culture, a fact made evident by the retention of African influences in black music, dance, language and speech patterns.<sup>2</sup> Afro-American slaves nevertheless came to share many values and beliefs with their owners, including Christianity.<sup>3</sup> An examination of the naming of slaves offers one way of

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<sup>1</sup> Sidney W. Mintz and Richard Price, “An Anthropological Approach to the Afro-American Past: A Caribbean Perspective,” Institution for the Study of Human Issues, *Occasional Papers in Social Change*, 2 (Philadelphia, Pa., 1976), 4–11; Melville Herskovits, *The Myth of the Negro Past* (Boston, 1958); George M. Foster, “Peasant Society and the Image of Limited Good,” *American Anthropologist*, 67 (1965): 293–315; Robert Redfield, *The Primitive World and Transformation* (Ithaca, N.Y., 1953), 51–58. Mintz and Price suggested that a simple encounter model positing a cultural unity of West Africa whose elements could be transferred or modified in the New World must be rejected as a useful approach to the problem. They maintained that scholars may be better served by focusing more on values and attitudes that define social relations—the underlying principles that govern family, kinship, and communal activities—as the source of cultural unity in West Africa and cultural continuity with an African past.

<sup>2</sup> See John Blassingame, *The Slave Community: Plantation Life in the Antebellum South* (New York, 1972), 1–40; Peter Wood, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974), 167–91; Charles Joyner, *Down by the Riverside: A South Carolina Slave Community* (Urbana, Ill., 1984), 196–224; and Lynne Emery, *Black Dance in the United States from 1619 to 1970* (Palo Alto, Calif., 1972). Lawrence Levine, *Black Culture and Black Consciousness: Afro-American Folk Thought from Slavery to Freedom* (New York, 1977), 3–5, 60–61, addressed the issue of Afro-American cultural transformation directly by suggesting that, regardless of its origins, slave music was a “distinctive cultural form” that contained not so much African survivals as transformations. Eugene Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1972, 1974), 309–13, 318–19, 322–24. Herbert Gutman, *The Black Family in Slavery and Freedom, 1750–1925* (New York, 1976), 194–201. Gutman most nearly followed Mintz and Price’s agenda by searching for the hidden structures that governed slave family life.

<sup>3</sup> See Robert Fogel and Stanley Engerman, *Time on the Cross: The Economics of American Negro Slavery*,

viewing the cultural transformation in the early generations of black slavery and focuses on a stage of the process that scholars have often neglected.

This examination of the names and naming practices of slaves considers the subject from three perspectives. First, plantation records reveal the size, content, and continuity of the pool of names that slaves used. As parents selected names for their children, they may have reflected religious convictions, cultural antecedents, or contemporary heroes and events. Shifts in naming practices may indicate changes in the values and world view of the slaves. Second, the rules a society uses in the selection and transmission of family names indicate the value that the parent or the individual selecting the name attached to the preservation of ties to the namesake. As Daniel Scott Smith observed, "The desire to recognize familial continuity spans a range of intensities and a variety of naming practices in different societies."<sup>4</sup> Some cultures express continuity by linking the child to the grandparent or extended kin, others by linking the child to the parent. With slaves, the study of naming practices includes a third dimension that speaks directly to the issue of acculturation: the extent to which the names used by slaves represented an agenda of names and naming patterns determined by owners or, more broadly, by white American practices, as well as the extent to which the names reflected African naming systems retained or transformed by life under the institution of slavery.<sup>5</sup>

This study draws on the records of the slave populations on the Ball family rice plantations, located on the Cooper River in St. John's Berkeley Parish, South Carolina. Between 1698 and 1865, the Ball family owned at various times nine rice plantations in the Low Country that are partially or fully documented. Comingtee, the home plantation, was inherited in 1698 and remained in the Ball family until 1896. Stoke, a quarter of Comingtee, was a separate unit between 1805 and 1840.

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2 vols. (Boston, 1974), 1: 126–57. Fogel and Engerman found acceptance by Afro-Americans of both a Protestant work ethic and a system of kin organization that emphasized the nuclear family. See also Genovese, *Roll, Jordan, Roll*, 162–63, 283. Genovese argued that the religion of slaves manifested many African traits and exhibited a continuity with African ideas; it also produced a sense of self-worth in a belief system that was shared with the slaveowners.

<sup>4</sup> Daniel Scott Smith, "Child-Naming Practices, Kinship Ties, and Change in Family Attitudes in Hingham, Massachusetts, 1641 to 1880," *Journal of Social History*, 18 (1985): 548. The Hingham population is white.

<sup>5</sup> Studies of slave-naming practices include Newbell Niles Puckett, "Names of American Negro Slaves," *Studies in the Science of Society*, George Murdock, ed. (New Haven, Conn., 1937): 471–94; Stephen Gudeman, "Herbert Gutman's *The Black Family in Slavery and Freedom, 1750–1925*," *Social Science History*, 2 (1979): 56–65; Mary Beth Norton, *Liberty's Daughters: The Revolutionary Experience of American Women, 1750–1800* (Boston, 1980), 85–87; Darrett Rutman and Anita Rutman, *A Place in Time: Explicatus* (New York, 1985), 97–103; Allan Kulikoff, *Tobacco and Slaves: The Development of Southern Cultures in the Chesapeake, 1680–1800* (Chapel Hill, N.C., 1986), 325–26. Four studies of slave names have focused on the Carolinas: Wood, *Black Majority*, 181–86; Joyner, *Down by the Riverside*, 217–22; Orville Vernon Burton, *In My Father's House Are Many Mansions: Family and Community in Edgefield, South Carolina* (Chapel Hill, N.C., 1985), 165–66; and John C. Inscoc, "Carolina Slave Names: An Index to Acculturation," *Journal of Southern History*, 49 (1983): 527–54. Both Wood and Joyner are particularly interested in the use of African "day-names" in eighteenth and nineteenth-century South Carolina. Inscoc uses names to date a transition to biblical names among Carolina slaves in the early nineteenth century. Gutman's work and my own have focused on the use of family names by slaves. See Gutman, *The Black Family*, 185–201; and Cheryll Ann Cody, "Naming, Kinship and Estate Dispersal: Notes on Slave Family Life on a South Carolina Plantation, 1786 to 1833," *William and Mary Quarterly*, 3d ser., 39 (1982): 192–211.

Kensington and Hyde Park were built in the 1740s and remained in the family until 1842. Limerick was purchased in 1764 and retained until 1891. Jericho (or Back River) was in the family from 1774 through 1842, Quinby from 1817 to 1842, and Cedar Hill and Halidon Hill were acquired in 1837 and 1842, respectively. The history of the plantations' settlement and the growth of their populations is detailed elsewhere, but several critical features need to be noted for the analysis that follows.<sup>6</sup> The birth and death registers of the Ball plantations span the years 1720 to 1865. Although extremely rich in some ways, these registers do not regularly record the name of a child's father. In some cases, this information can be gleaned from other sources, but a full genealogical reconstruction of the slave populations cannot be accomplished. The Ball records are nonetheless among the best documentation available of the earliest generations of American-born slaves.

The Ball family's slave population was subjected to few of the disruptions that came to other slaves through sales and the transmission of property from generation to generation. Before 1770, the Balls purchased slaves to increase the population. After 1770, additions other than by births came from a marriage gift in 1774, the purchase of a Tory cousin's slaves in 1784, and the inheritance of 140 slaves in 1812. In each instance, the source of the slaves was another member of the Ball family. A major disruption of the slave population came with the auction of 366 slaves from the estate of John Ball, Sr., in 1819. His sons purchased 126 of the slaves, as John Ball, Jr., noted, "because they were related by connection with those that were owned by me before the sale."<sup>7</sup> After 1840, the slaves at three plantations were no longer included in the surviving registers. As a result, a portion of the population disappears from this reconstruction. During the nineteenth century, the evidence becomes fuller with a column added to the register in which the date of death was recorded. Only for the last fifteen years, 1849 to 1865, and for one plantation, Comingtee, were the fathers of children regularly recorded. Beginning with these registers, 620 maternal histories were reconstituted. These

<sup>6</sup> See my unpublished dissertation, "Slave Demography and Family Formation: A Community Study of the Ball Family Plantations, 1720–1896" (Ph.D. dissertation, University of Minnesota, 1982), for details of the years of documentation for each site. On the impact of the 1819 sale of John Ball's slaves, see Cheryl Ann Cody, "'Painful Though It Might Be': Inheritance Practices and Slave Families on the Ball Plantations," *Working Papers of the Social History Workshop*, 86–3, Department of History, University of Minnesota; and Cody, "Slave Demography and Family Formation," 323–57.

<sup>7</sup> More than 600 maternal slave histories were reconstituted from plantation records of the Balls housed in several archives, including *Plantation Journal* 1720–1782; and *Blanket and Cloth Book* 1833 to 1844, both in *Miscellaneous Papers*, Private Collection of Mr. Elias Ball (now at the South Carolina Historical Society); *John and Keating S. Ball Plantation Books*, vols. 1–10; and *William J. Ball Plantation Books*, vols. 1–2, both in *Southern Historical Collection*, University of North Carolina, Chapel Hill. *Blanket Book and Register of Isaac Ball for Midway, Limerick, Jericho, Quinby and Hyde Park* (ca. 1804–1821); and *Clothing and Blanket Book of John Ball's Slaves, 1830–1833*, both in *Ball Family Papers*, South Carolina Historical Society, Columbia. *Keating S. Ball, Comingtee Plantation Book, 1849–1896*, *Ball Family Papers, 1696–1800*, South Caroliniana Library, University of South Carolina, Columbia. *Keating S. Ball Plantation Book, 1860–1867*, *Ball Family Papers, 1792–1834*, William Perkins Library, Duke University, Durham, N.C. Record series include: *Wills*; and *Inventories*, Charleston District; South Carolina Department of Archives and History, Columbia. The first register runs from 1720 to 1735 and lists the names of slaves born at Comingtee and slaves purchased by Elias Ball during each year. The second register spans 1735 to 1785 and for each child lists the date of birth, child's name, and mother's name, noting occasionally the death of the child. A third register, which overlaps the second, includes births from 1735 to 1817 with similar information.



**TABLE 1**  
**Leading Names for Ball Family Children**

Rank	Name	N	Percent	Cumulative Percent
Sons (N = 76)				
1st	John	21	27.6	27.6
2d	Elias	10	13.2	40.8
3d	William	9	11.8	52.6
4th	Isaac	6	7.9	60.5
Daughters (N = 76)				
1st	Elizabeth-Eliza	11	14.5	14.5
2d	Anne	8	10.5	25.0
3d	Eleanor	7	9.2	34.2
3d	Lydia	7	9.2	43.2
5th	Martha	5	6.6	49.8
5th	Mary	5	6.6	56.4

histories were supplemented by distribution lists, estate inventories, division lists, and other documents that systematically enumerated populations in an attempt to locate evidence on the fathers of children. If a woman was linked in these documents with the same man over the period of her childbearing years without breaks in either her history of births or her linkage to the man, it was assumed that he fathered her children. Though somewhat unorthodox, this method takes advantage of the owner's practice in recording slave names by modifying the names of women with the names of their husbands. By this method, I identified the husbands of 194 women or about one-third of the families.

The analysis of slave names uses four sets of the Ball slave reconstitution. One set includes the names of 2,344 slaves born on the plantations who lived long enough to receive a name in the birth register. This set is used in the analysis of the content and continuity of the name pool itself. The second set uses only maternal histories with full death registration and includes only families begun after 1800 and completed in 1865. This subset of families is used in the analysis of patterns of necronymic naming. The third set uses only families in which the name of the father or fathers of all children are known and includes a total of 194 families. A final subset of this third set includes only families in which all four grandparents are known. The last two data sets are used in the analysis of kin-naming patterns among slaves.

TO ASSESS THE IMPACT OF OWNER NAMES AND NAMING SYSTEMS on slaves, I turn first to the naming practices of the Ball family.<sup>8</sup> Members of the Ball family chose the

<sup>8</sup> See John and Keating S. Ball Plantation Books, vol. 5, Southern Historical Collection, University of North Carolina, Chapel Hill; Anne Simons Deas, *Recollections of the Ball Family of South Carolina and the Comingtee Plantation* (Charleston, S.C., 1978).

names for their own sons and daughters from a small group of family names. More than 60 percent of the sons received one of four names: John, William, Elias, or Isaac. John and William were common English names and popular among white American populations in both the eighteenth and nineteenth centuries.<sup>9</sup> Both Elias and Isaac were first names that were somewhat unusual for white populations of the Low Country, but each name held significance in the family history of the Balls. Elias I was the first member of the Ball family to immigrate to South Carolina in the late 1690s. He was the most commonly shared ancestor and the source of the family's wealth, and thus his name was important in a society that emphasized both kinship and property. Isaac, whose death at age twenty-one made him both a tragic and romantic figure in the family history, was also a popular namesake for Ball children, tapping a budding strain of sentimentalism in the planter family.<sup>10</sup>

Although not as restricted as the pool of male names, the group of common names for daughters was small. Six names, Elizabeth, Anne, Eleanor, Lydia, Martha, and Mary, accounted for 56 percent of the names selected for Ball daughters. Elizabeth, Anne, and Mary were among the leading English names for women and also were dominant in eighteenth and nineteenth-century American populations.<sup>11</sup> Lydia and Eleanor were perpetuated both as kin names and as namesakes because of the attributes of the first American-born ancestors to bear the names. Lydia Child Chicken's marriage to Elias II came after years of unrequited love, and she was thus a romantic figure in the family's history.<sup>12</sup> Eleanor Ball was the best-known individual in the family lineage, male or female. She was the recipient of a necronymic name, that is, she shared the name of her half-sister, who had died at age sixteen, seven years before Eleanor's birth. The only child of her father's second marriage to survive to adulthood, Eleanor married Colonel Henry Laurens. Her marriage not only linked the family to power and wealth, it also produced four children of prominence: John Laurens, a hero of the Revolutionary War; Henry II, a wealthy planter and politician; Martha, the wife of Dr. David Ramsey, the first historian of the state; and Mary Eleanor, the wife of Governor Charles Pinckney. By choosing Eleanor as the namesake for their daughters, the Balls perpetuated a family name and at the same time linked the family to the powerful political past of the Laurens, Ramseys, and Pinckneys.

The Balls relied heavily on family names, especially those of the paternal lineage, in selecting both first and middle names. By the nineteenth century, middle names had come into common use, increasing the ability of parents to perpetuate family names. Since 1714, the Balls had used the middle name Coming for sons named John to make clear that the child shared the name of John Coming, from whom the Balls originally inherited their land in South Carolina, as well as to continue the surname of the first owner of Comingtee, albeit transposed as a middle name. The first of the Balls to use middle names consistently were John Ball and his

<sup>9</sup> Smith, "Child-Naming Practices," 565; Rutman and Rutman, *A Place in Time: Explicatus*, 86.

<sup>10</sup> Deas, *Recollections of Comingtee*, 72–77.

<sup>11</sup> Smith, "Child-Naming Practices," 566; and Rutman and Rutman, *A Place in Time: Explicatus*, 87, 99.

<sup>12</sup> Deas, *Recollections of Comingtee*, 66–69.

**TABLE 2**  
**Percentage of Ball Family Children Sharing a Kin Name**  
**by Gender, Family Size, and Kin Connection**

	SONS						DAUGHTERS					
	<i>1-2</i>			<i>3 or more</i>			<i>1-2</i>			<i>3 or more</i>		
	N	Percent	N	Percent	N	Percent	N	Percent	N	Percent	N	Percent
Father Paternal	8	75.0	14	100.0	22	90.9	13	53.8	12	91.7	25	72.0
Grandfather (FAFA) Maternal	8	37.5	14	78.6	22	63.3	13	23.1	12	66.7	25	44.0
Grandfather (MOFA)	8	12.5	14	57.1	22	40.9	13	30.8	12	75.0	25	52.0
<div> <i>Percent of First and Second Sons Sharing Name</i> </div> <div> <i>Percent of First and Second Daughters Sharing Name</i> </div>												
	N			FAFA			Both			MOFA		
				Neither Parent nor Grandparent								
1st Son	22	68.2	27.3	{13.6}	4.5	13.6	25	44.0	32.0	{8.0}	28.0	0
2d Son	19	36.8	26.3	{ 5.3}	5.3	36.9	22	22.7	18.2	{ 0}	13.6	45.5

second wife, Martha Carolina Swinton Ball. Whereas only one of the five children of John's first union, with his cousin, received a middle name, John and Martha gave ten of their eleven children middle names. The middle names selected fell into three categories: traditional first names, family surnames, and Latin names for numerals, such as Septimus. John and Martha gave five of their daughters middle names that were traditional first names, including the first set of twins, Carolina Olivia and Martha Angelina, each of whom shared one of their mother's names.<sup>13</sup>

One indication of the preference for paternal kin names is the frequency with which sons received the names of their fathers. Six of the eight families with one or two sons passed on the father's name to the next generation. All fourteen families with three or more sons named a son for his father. Members of the Ball family frequently used the paternal grandfather's name. In families with only one or two sons, three of eight named a child for his paternal grandfather. Among large families, the use of the paternal grandfather as a namesake was more prevalent, with 78.6 percent including a son who shared the name of his father's father. Maternal grandfathers rarely saw their name used for a child in families with one or two sons, but, again among larger families, more than half named a male child for his maternal grandfather. Considered together, nine out of ten families transmitted the name of the father to the next generation. About six of ten named a son for a paternal grandfather and four in ten for the maternal grandfather. Thus, in naming sons, the Balls relied on kin names with great frequency and preferred the immediate connection of father to son.

Because parents could not anticipate the number of children of each gender they would produce, the order in which they used kin names reveals not only the intensity of kin naming but also the priorities held. In selecting the names of first sons, members of the Ball family turned to the names of the fathers in more than two-thirds (68.2 percent) of the families. First-born sons who did not receive the names of their fathers (27.3 percent) often received the names of their paternal grandfathers but only rarely those of maternal grandparents (4.5 percent). Even though parental and grandparental names were chosen less frequently for second sons, families that had failed to name a first son for his father used the opportunity of the birth of a second boy to perpetuate the father's name. Again, the families seldom turned to the maternal grandfather as the source of a second son's name.

Kin naming was not practiced as intensely for daughters as it was for sons. Slightly more than half (53.8 percent) of families with one or two daughters named a child for her mother. Among larger families, however, more than nine in ten used the mother's name. The maternal grandmother and the paternal grandmother served as namesakes with nearly equal frequency. Among families with

<sup>13</sup> The use of middle names also brought a new dimension to necronymic naming. Among the children of John Ball, Sr., and Martha Carolina Swinton, Angelina Mailda received the middle name of her recently deceased sister as her first name, and Edward William shared the two names of his deceased half-brothers. Other children were given surnames as their middle names, including Coming, Swinton, and Splatt. The use of Latin names also increased the opportunities for necronymic naming. One son, who shared the first name of his deceased half-brother, was called Elias Octavius.

only one or two daughters, 30.8 percent named one girl for her maternal grandmother, while 23.1 percent named a girl for her paternal grandmother. In families with three or more daughters, nine of twelve perpetuated the maternal grandmother's name and eight of twelve the name of the paternal grandmother.

A study of the naming priorities for first and second daughters reveals a Ball family preference for replication of the name of the mother over that of either grandmother, but the pattern of parental naming overall is weaker than that found for sons. All first-born daughters who shared the name of their mother or a grandmother did so in the following order of frequency: mother, maternal grandmother, and paternal grandmother. The small number of cases, the number of shared names, and the relatively slight differences detected suggest, however, that no clear-cut pattern of preference between parental or grandparental or between maternal and paternal lineage can be discerned. Furthermore, while all first-born daughters received a kin name, nearly half of the second-born daughters were given a name other than that of their mother or grandmother.

In selecting names for their children, the Balls valued perpetuation of the kin name above preservation of the individual identity of a child. One indication of this attitude is the frequency of necronymic sibling naming. David Scott Smith noted a trend away from the use of sibling necronyms by the nineteenth century among the Hingham population.<sup>14</sup> The Balls, however, continued to reuse the name of a dead child, taking advantage of six of twenty-six opportunities for replacement (23.1 percent) presented by the birth of a child of the same gender.

The intensity of kin naming among the Ball family and the emphasis placed on perpetuating the paternal lineage can be seen in the names chosen for the children of William James Ball. In 1842, William James married Julia Cart, and, over the next eight years, the union produced five sons. The first four boys received the four family names used most frequently for male children and in order of kin relationship. The first son was named for his father, William James; the second for his paternal grandfather, Isaac; the third for his paternal great-grandfather, John; and the fourth for his paternal great-great-grandfather. In addition to directly replicating the paternal line, these four boys also shared the names of other paternal kin. Julia's father was John Cart; thus, the maternal grandfather also received a namesake among the Ball children. Because no daughter was born to William James and Julia, there was no opportunity to perpetuate Julia's name or the names of the grandmothers.

A second marriage allowed William James to broaden the scope of his use of kin names. Four years after Julia's death, he married his first cousin, Mary Huger Gibbs. This union produced one son and six daughters. The son shared the name of his maternal grandfather. The Balls were able to use the maternal grandfather's

<sup>14</sup> Smith has altered his interpretation of the nineteenth-century decline in the practice of necronymic naming. He originally equated the practice with a parental failure to recognize the child as a unique being; Daniel Scott Smith, "Child Naming Patterns and Family Structure Change: Hingham, Massachusetts, 1640–1880," *Newberry Papers in Family Community History*, 76–5 (January 1977), 6. Smith later argued that, as death became romanticized, the intensity of grieving increased, and the connection between child and name more binding, families felt it was no longer appropriate to reuse a name. Smith, "Child-Naming Practices," 546–47.



**TABLE 3**  
**Children of William James Ball**

Rank	Gender	Gender-Rank	Name	Relationship	Status
<i>wife Julia Cart (married 1842)</i>					
1st	Son	1st	William James	Father	living
2d	Son	2d	Isaac	Paternal Grandfather (FAFA)	dead
3d	Son	3d	John	Paternal Uncle Paternal Great-Grandfather (FAFAFA)	dead dead
4th	Son	4th	Elias	Paternal Uncle Paternal Great-Great-Grandfather (FAFAFAFA)	living dead
5th	Son	5th	Francis Guerrin	Paternal Great-Uncle Paternal Cousin	dead unknown
<i>wife Mary Huger Gibbs (married 1862)</i>					
6th	Daughter	1st	Eliza Catherine	Paternal Grandmother (FAMO)	living
7th	Daughter	2d	Maria Louisa	Paternal Aunt Maternal Grandmother (MOMO)	dead unknown
8th	Daughter	3d	Jane	Paternal Great-Grandmother (FAFAMO)	dead
9th	Son	6th	Mathurin Guerrin	Maternal Grandfather	unknown
10th	Daughter	4th	Mary	Mother	living
11th	Daughter	5th	Lydia Child	Paternal (distant)	—
12th	Daughter	6th	Eleanor	Paternal (distant)	—

name for their first-born son because William James had completed his naming agenda for his male ancestry during his first marriage. The paternal line dominated the names selected for the six daughters. The first daughter shared the name of her paternal grandmother. The maternal line was not represented among William James's children until the seventh child (the second daughter, Maria Louisa, named for her maternal grandmother). The third daughter, Jane, shared the name of her paternal great-grandmother, the fourth was named for her mother, and the fifth was Lydia Child, the infant's paternal great-great-grandmother. The sixth daughter received the name Eleanor, from a kin relationship even more distant.

THE MOTIVATION OF SLAVEOWNERS IN THE SELECTION OF NAMES for their own children seems fairly simple—the perpetuation of kin names, particularly, paternal kin—but the names chosen for slave children stem from a far more complex set of influences and values, some held by owners and others by slaves. For their part, owners needed to identify each slave by a single given name to

simplify the allocation of tasks and provisions. If this need had totally governed the selection of names for slave children, it would have meant that no slave would have shared a first name with another slave on the same plantation, and no child would have shared the name of a parent, a living grandparent, or a cousin. On large plantations, this rule would require a broad pool of names, equal to the total population.

The desire to identify slaves uniquely accounted for the variety of names and the diversity of sources that the Balls used when they purchased slaves or named the first generation born in South Carolina.<sup>15</sup> It conflicted with any desire slaves may have had to use family names for their children. Even so, the first generation of South Carolina-born slaves on the Ball plantations seems to have asserted its influence by using kin names for its children.<sup>16</sup> Indeed, the slaves made use of family names not only between the generations but among extended kin of the same generation as well, thus quickly undermining an effort to create a unique identity for each slave. When slaves on the same plantation shared the same first name, some form of modification was added to one or both names to distinguish between the two individuals. Name duplication was a common but easily resolved problem when a father and son, or grandfather and grandson, shared a name. They would be identified, for instance, as “Big Pompey” and “Little Pompey.” Other modifiers could be equally descriptive, including color, place of birth (either Africa or another plantation), or occupation. A comparison of the use of name modifiers can be made between a list from 1755 to 1758 of slaves receiving blankets at Comingtee and a blanket list from 1835 to 1837 at the same site. Although few slaves on the eighteenth-century list shared a name, Comingtee plantation recorded “Ebo Sylvia” and “Ebo Dy.” Surnames were applied to three slaves, Jenny Buller, Tom White, and Hannah Shubrick. Age was used to differentiate six slaves. By the nineteenth century, modifiers were more frequently applied and, in more than half of the instances, were given to slaves who did not share the first name of another slave. Nearly two-thirds (63.7 percent) of the adult male slaves had some form of modifier applied to their names, with occupation and kinship accounting about equally for the type of modification. Seven out of ten female adults had their

<sup>15</sup> The number of plantations, size of slave populations, and regular absenteeism of the owners all make it unlikely that the Balls continued to select the names of slave children after the first generation born in South Carolina. For example, the inventory of the estate of John Ball, Sr., in 1817 lists 669 slaves at nine plantations and in Charleston. In Burton’s recent study of Edgefield County, South Carolina, he avoided taking a position on whether slaves or owners named slave children; Burton, *In My Father’s House*, 166. Only one of twenty-four entries of births to slave mothers in Thomas Chaplin’s diary of Tombee plantation notes that Chaplin named the slave infant. Theodore Rosengarten, *Tombee: A Portrait of a Cotton Planter; with the Journal of Thomas B. Chaplin (1820–1890)* (New York, 1986), 330, 358, 364, 496, 501, 504, 533, 536, 589, 612, 633, 635, 640, 669, 674, 695, 711–12.

<sup>16</sup> There is some evidence to suggest that the selection of a child’s name did not rest solely with slave parents. Joyner argues that some slaves were named by their grandparents, a continuation of an African practice; *Down by the Riverside*, 220–21. A notation made by Keating S. Ball in 1876 lends support to the view that the selection and ritual recording of a child’s name may have included a broader kin, in this instance the child’s maternal grandmother. He recorded, “Mother Judy Johnson formerly of this place now at Hagan St. Thomas; Nov 22; male; Ben entered at the request of Dorcus Pinckney the grandmother”; Keating S. Ball, *Comingtee Plantation Book, 1849–1896*.

names modified as well. Kinship was most often the form of modification, with the name of the woman's husband or her mother chosen with about equal frequency.

In a society in which one's history and position were transmitted orally, the selection of the name of a child may have held more significance than in a society with a written history. The selection of an African "day-name," for example, would give a child a name used solely by blacks in the community and would serve also as a reminder of an African past. Sharing a kin name was a useful device to connect children with their past and place them in the history of their families and communities. Because both slaveowner and slave used shared names as evidence of kinship, the Ball slaves appear to have avoided names uniquely associated with their male owners and the Ball lineage. This pattern of avoidance did not hold for female slaves, whose maternal parentage would be unquestioned, nor was it practiced when the male owner's name was common, since sharing such a name would not necessarily identify the owner as the father of the child.<sup>17</sup>

In addition to these patterns of choice and avoidance, the Ball slaves' conversion to Christianity added an additional dimension to naming practices. The lessons of the Bible presented not only a new or revitalized pool of names for slaves but also a model of family organization that stressed male roles and paternal ties. The selection of a biblical name and the emphasis on the paternal line in the choice of a kin name brought slaves closer in some ways to the familial values of owners.

The evolution of these influences on naming practices can be seen in the family history of Windsor and Angola Ame, slaves on the Comingtee plantation.<sup>18</sup> Windsor and Angola Ame had two sons and five daughters between 1743 and 1758. Each of the children received a name chosen, most likely, by the plantation owner or, at least, at his suggestion. The first son and daughter were given names that scholars label Anglicized day-names. Christmas, born on 25 December 1743, received a name appropriate for his date of birth. He was followed by a sister, Easter, born the Saturday before Easter Sunday. These names preserved the African form of day-naming or naming a child for an event, but the content of the name was that of a Christian religious holiday.<sup>19</sup> The selection of these names indicates either direct owner interference or the recognition of the Christian and work holidays of Christmas and Easter by Windsor and Angola Ame, who applied the content of the holiday to an African form.

Although slaves generally avoided the use of male names uniquely associated with their owners, female slaves occasionally received the name of an owner's sweetheart, wife, or daughter. Windsor and Angola Ame's second daughter was named Lydah (Lydia), sharing a form of the name of her owner's bride, Lydia Child Chicken Ball.<sup>20</sup> The appearance of Lydia as a slave name at Comingtee

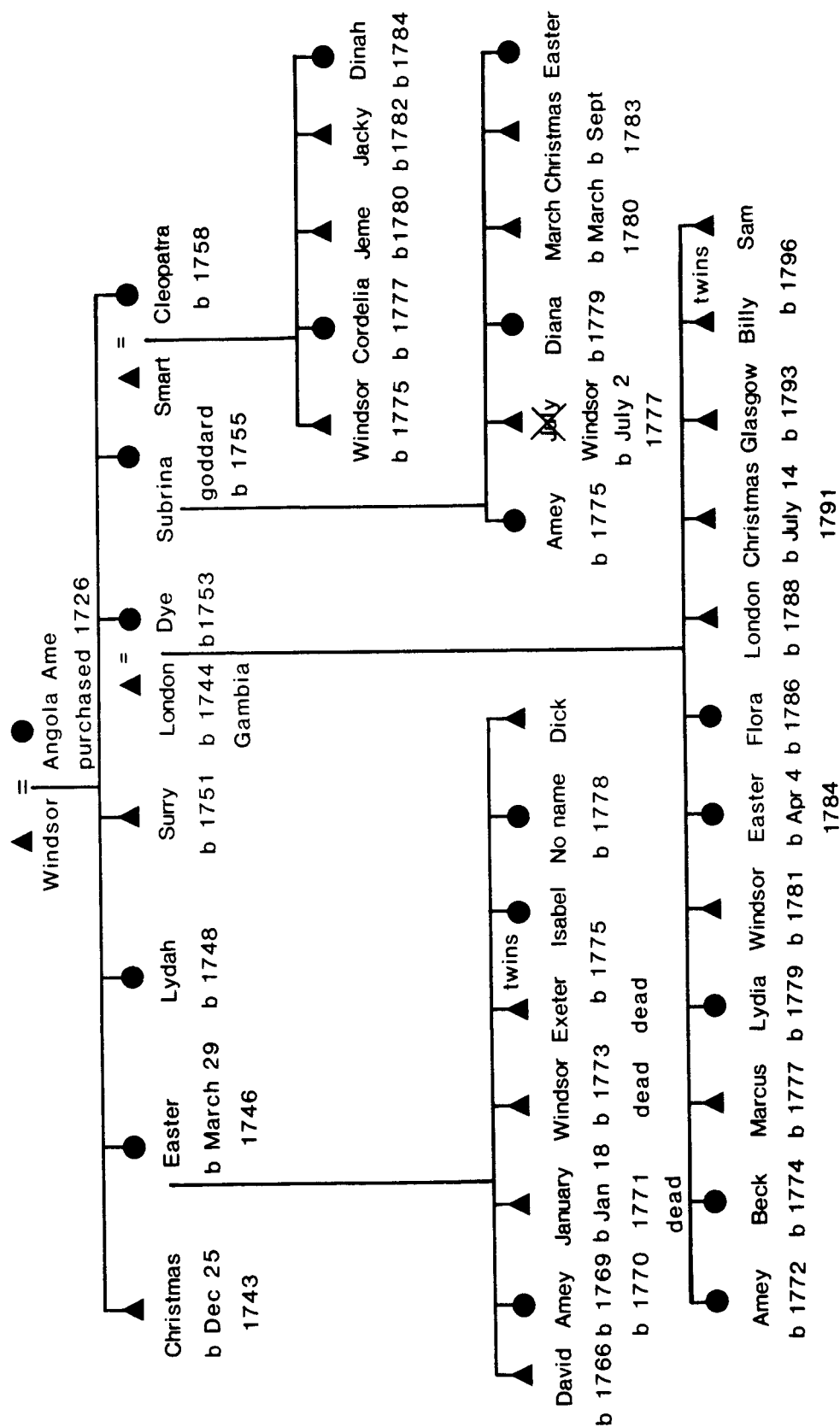
<sup>17</sup> Inscoc, "Carolina Slave Names," 539–40. Inscoc detected a pattern of avoidance of owner's names on the plantations he studied.

<sup>18</sup> Angola Ame was brought to Comingtee in 1726 when she was probably less than ten years old. Windsor does not appear in either the birth or purchase records for 1720 through 1735, suggesting that his birth occurred before 1720 or that he was purchased sometime between 1735 and 1743. Either scenario would be consistent with the record of the births of his children.

<sup>19</sup> Wood, *Black Majority*, 181–82.

<sup>20</sup> Deas, *Recollections of Comingtee*, 66–71.

**FIGURE A**



suggests that either Windsor and Angola Ame or Elias II used the opportunity of the birth of a female child to name a child for the owner's bride.

The sources for the names of the next four children are more literary than personal or religious and reflect the breadth of sources used by owners for slave names. Windsor and Angola Ame's second son, Surry, like his father, was given an English place name. Diana (Dye) is the Latin name for the moon goddess and also a common English name. Subrinagoddard (later shortened to Sabrina, Sabina, and possibly Binah) may have its origin in German or Scandinavian mythology. Cleopatra (later Patra), for the Queen of the Nile, was one of the few female figures from ancient history available as a namesake.

This initial group of nine names (Windsor, Angola Ame, Christmas, Easter, Lydah, Surry, Dye, Subrinagoddard, and Cleopatra) was repeated among the children of the next generation, demonstrating that slaves reused owner-influenced names as kin names and in this way made them their own. The names of Windsor and Angola Ame were used by their children in naming the couple's grandchildren, thus connecting the second generation of American-born slaves with their African-born grandparents. Each of the four daughters who survived to bear children named a son for the maternal grandfather, Windsor. Easter's third son, who shared the name Windsor, died shortly after birth in 1773; two years later, Cleopatra's first son, Windsor, also died as an infant. When Subrinagoddard's two sisters were unable to perpetuate their father's name, she changed the name of her oldest son from July (he was born on 2 July 1777) to Windsor. Dye and London's second son, born in 1781, also shared the name of his maternal grandfather. The efforts these four women made to perpetuate the name of their father, including changing the name of July, indicates that they placed a high priority on the preservation of family names. This desire may have been intensified by the elder Windsor's death prior to 1778. In reusing the name of their father for their sons, the four sisters attempted to ensure that his memory would be kept alive into the next generation.

The daughters of Windsor and Angola Ame also chose their mother as a namesake for their daughters. Easter, Dye, and Subrinagoddard all named their first-born daughters for Angola Ame. Significantly, none of them named a daughter for herself. Cleopatra did not reuse her mother's name, although she had two opportunities. Perhaps she felt that, since a living namesake existed, she could carry out another naming agenda.

Unlike the Balls, whose naming practices reflected depth but little breadth, the slaves emphasized kin ties that included both generational depth and breadth. Dye named daughters for her dead sister Lydah and for Easter, and a son for her brother, Christmas. She named a son London, after his father. Although Easter and Cleopatra seem to have been less involved in kin naming, Subrinagoddard gave five of her six children names for maternal kin. She named a daughter for each of her older sisters and a son for her only living brother.

Day-names and event names appeared seven times among the third generation of this family, illustrating the use of Anglicized day and event names and the perpetuation of this type of name as a kin name. July (later, Windsor) and March,



the sons of Subrinagoddard, were born in the months for which they were named. Similarly, the name of Easter's son January was consistent with his month of birth. Although Windsor and Angola Ame's children Christmas and Easter received their names because of their dates of birth, the names were perpetuated as kin names. Sons of Subrinagoddard, and those of London and Dye born in July and September, were named Christmas. The name Easter was perpetuated as a kin name during April of 1784 and again in 1785.

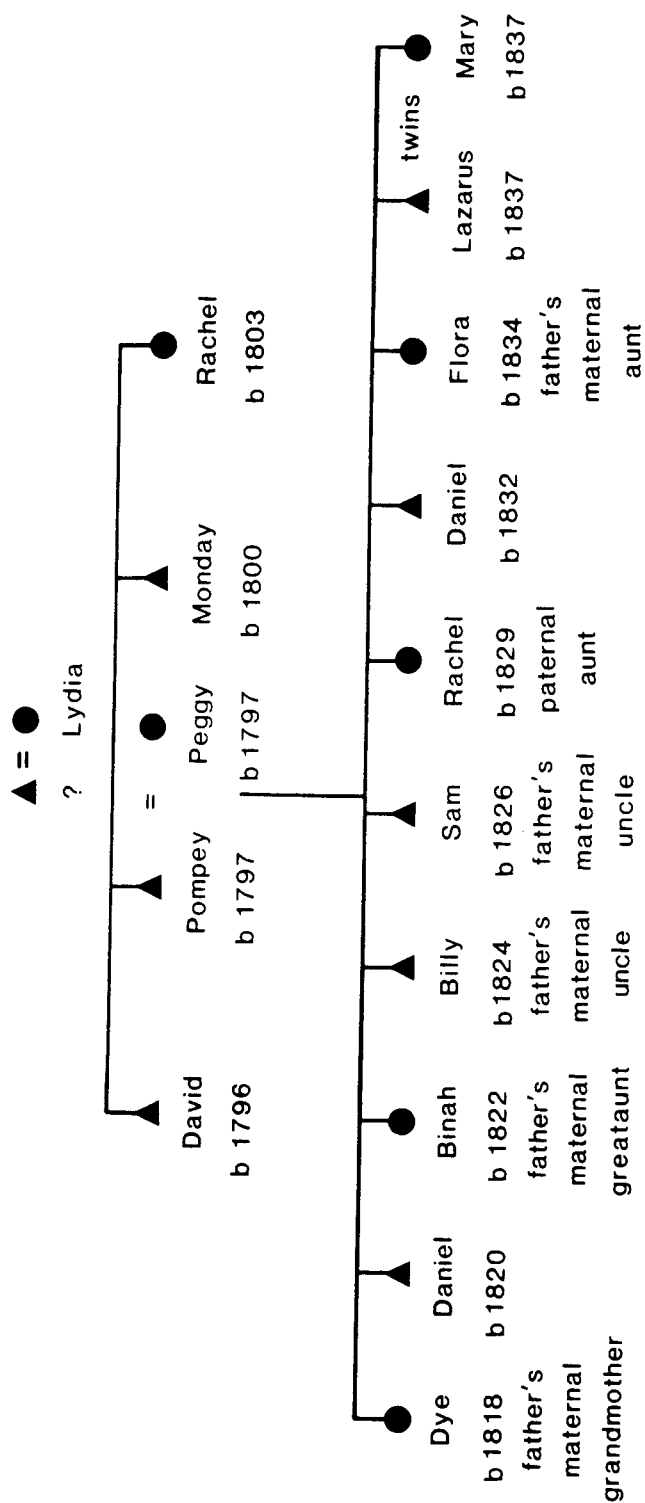
The daughters of Windsor and Angola Ame were successful in transmitting kin names through the maternal line. Their brother Christmas probably shared a similar desire to name his offspring for his parents and siblings, but, unlike his sisters, Christmas had few opportunities to find a spouse on the plantation, and he may not have fathered a child in a recognized union at Comingtee. Between 1750 and 1780, Christmas lived on a plantation where the adult males outnumbered females, and it may have been difficult for him to form a stable union. A list of Ball slaves in 1784 places him at Comingtee, where the ratio was 210 males per 100 females, and men over the age of fifteen outnumbered women of the same age group twenty-three to seven. The list also placed him as head of a family that included the children of his deceased sister, Easter.<sup>21</sup>

The inability of some male slaves to form unions on their home plantations means that any family names they were able to pass on to their children would have been on another plantation and would not be reflected in the Ball family records. Although this gap in the evidence produces an apparent bias in favor of the recognition of maternal kin, even for instances in which both spouses were born on Comingtee, no paternal preference in kin naming as seen among the Ball family can be found during this early period (see Tables 8 and 9).

Evidence suggests that, by the fourth and fifth generations, slaves began gradually to shift kin naming from a bilateral system to one that emphasized the paternal line. Lydia, the daughter of London and Dye, was the mother of four children: David, Pompey, Monday, and Rachel. The records do not reveal their father or fathers nor do the children bear names that were shared by maternal kin. In selecting the names of their children, Lydia's son Pompey and his wife Peggy reflected a transitional system that emphasized the maternal line while beginning a shift in favor of paternal kin names. They named five of their ten children for paternal kin and turned to Pompey's mother's family for four of their children's names. They named Dye for Pompey's maternal grandmother. Billy and Sam were named for Pompey's maternal uncles and Flora for his maternal aunt. Only one daughter, Rachel, shared the name of a relative in the immediate paternal line, specifically, Pompey's sister who had died. They gave Binah a common name for slave girls on the Ball plantations, but, again, they may have used Pompey's maternal great-aunt, Subrinagoddard, as a namesake. When selecting names from the extended kin group, Pompey chose the names of his mother's family. No child of Pompey and Peggy shared the name of a parent or grandparent. Kin names

<sup>21</sup> "A List of Negroes the Property of Elias Ball, Made the 12th Day of May 1784," Ball Family Papers, South Carolina Historical Society; Cody, "Slave Demography and Family Formation," 50–55.

FIGURE B  
THE FAMILY OF POMPEY AND PEGGY



**TABLE 4**  
**Persistence of Names Given at Birth for Male and Female Slaves Born on the Ball Plantations, 1720-1865**  
**by Gender and Cohort**

Birth Cohort	Total No. Named	Total No. of Names Used	New Names		1740-59		1760-79		1780-99		1800-19		1820-39		1840-65	
			Names	Percent	N	Percent	N	Percent	N	Percent	N	Percent	N	Percent	N	Percent
MALE CHILDREN (N = 1163)																
1720-39	32	31	31	80.6	24	77.4	24	77.4	23	75.0	21	66.7	21	66.7		
1740-59	42	37	29		22	75.9	22	75.9	22	75.9	15	51.7	9	31.0		
1760-79	73	59	44				35	79.5	33	75.0	28	63.6	20	45.5		
1780-99	86	64	21						15	71.4	12	57.1	4	19.0		
1800-19	285	163	88								44	50.0	31	35.2		
1820-39	402	150	25										4	16.0		
1840-65	243	108	16													
FEMALE CHILDREN (N = 1181)																
1720-39	26	21	21	95.2	20	95.2	20	95.2	20	95.2	20	95.2	18	85.7		
1740-59	42	32	29		23	79.3	23	79.3	22	75.9	19	65.5	16	55.2		
1760-79	90	66	42				36	85.7	36	85.7	35	83.3	31	73.8		
1780-99	77	57	20						10	50.0	8	40.0	6	30.0		
1800-19	273	143	70								38	54.3	26	37.1		
1820-39	396	137	29										9	31.0		
1840-65	277	115	12													
ALL CHILDREN (N = 2344)																
1720-39	58	52	52	86.5	44	84.6	44	84.6	43	82.7	41	78.8	39	75.0		
1740-59	84	69	58		45	77.6	45	77.6	44	75.9	34	58.6	25	43.1		
1760-79	163	125	86				71	82.6	69	80.2	63	73.3	51	59.3		
1780-99	163	121	41						25	61.0	20	48.8	10	24.4		
1800-19	558	306	158								82	51.9	57	36.1		
1820-39	798	287	54										13	24.1		
1840-65	520	223	28													

were used to link children born in the 1820s and 1830s with a more distant ancestry.

THE SIZE, CONTINUITY, AND COMPOSITION OF THE SLAVE-NAMING POOL developed out of these influences on the selection of slave names. While the Balls drew from a small pool of names, the slaves drew from a large one, a consequence of the desire of owners to provide each slave with a unique identity. As in the case of the children of Windsor and Angola Ame, the sources of these names were diverse. This need for a large pool of names may have enhanced the ability of slaves, at least among the first generation, to maintain African names as well as African practices of day-naming. For 1,163 male children born between 1720 and 1865, a total of 254 different names were used. The slightly larger number of 1,181 female children received their names from a smaller pool of 223 names. Prior to 1780, slaves born on the Ball plantations were given names that reflected the owner's desire for diversity. During these years, approximately three of four slaves (74.2 percent) received a name that had not been used before for an American-born child. Not all of these names were necessarily "new," in that some were used for slaves who had been purchased during that period, but they were new among slaves born on the Ball plantations. Between 1780 and 1800, the slaves began to name their own children, and they used a kin system. About two-thirds (66.1 percent) of the children were given names that had been used previously. The first two decades of the nineteenth century saw another shift in the structure of naming. But, after 1820, few names were given to slaves that had not been chosen continuously since they first entered the pool.

The chronological pattern is highly suggestive of the shift. Prior to 1780, slaves born on the Ball plantations received names that, in their diversity, failed to reflect generational ties. As was the case in the family of Windsor and Angola Ame, few children shared the name of a parent or, in fact, of any other living slave born on the Ball plantations. After 1780, the pattern was altered as a smaller percentage of "new" names was introduced and the use of kin names became more common. This was the second generation of American-born slaves to share the names of grandparents, aunts, and uncles. Between 1800 and 1820, more "new" names were introduced on the Ball plantations, but, after 1820, few names were brought into use, perhaps because the slight surge of names between 1800 and 1820 increased and diversified the pool of names to accommodate nearly everyone's naming agenda.

One measure of the continuity of names on the Ball plantations is the reuse of the group of names given to the first generation of American-born slaves for children born in the nineteenth century. Because this group of names was selected by owners in the interest of creating a distinct identity for each individual, the rate of persistence of these names indicates the long-term influence of owners on slaves' names. The repetition of names, family names in particular, depended on the size of slave families, the level of mortality, and the stability of the population under observation, and is difficult to measure; nonetheless, some comparisons are possible.

The method devised to capture the persistence of male and female names on the Ball plantations consists of recording the appearance of each name (results reported in Table 4). If a name was reused within forty years of its first appearance, it was counted as an “active” name. If a name was not reused within forty years, it was counted as removed from the “active” pool during the first twenty-year period it was not used for a child born on the plantations. For example, the name Caesar was used for one child born between 1720 and 1740. It was not reused during the next twenty-year period but appeared twice as the name of boys born between 1760 and 1780. Again, the next twenty years saw no child named Caesar, but two children born between 1800 and 1820, three between 1820 and 1840, and six between 1840 and 1865 shared the name. Caesar, then, was counted as a new name in the 1720 to 1740 period and as one of twenty-one of the original thirty-one names still active in 1865. Another classical name, Cato, was first used for a child born on the Ball plantations between 1760 and 1780. It was one of the forty-four male names that were “new” (had not been used previously for a child at birth) during that twenty-year period. “Cato” was reused during the next twenty years, but no slave born on the Ball plantations after 1800 was named Cato. Cato, then, was one of the two names first introduced between 1760 and 1780 and reused during the following period that was not used again after 1800.

Male and female names show a substantial difference in continual use on the Ball family plantations: male names were far more likely to disappear than were female names. In every period (see Table 4), more female names that were new to the pool of names were reused and remained in use up to the Civil War. Of greatest interest to the point under consideration is the persistence of the owner-influenced names that appeared before 1780. Of the thirty-one names given to male children between 1720 and 1739, two-thirds were active in 1865 and had been active for more than a hundred years. Female names once used for a slave on the Ball plantations continued to be reused with greater frequency, and 85.7 percent of those introduced between 1720 and 1739 remained active until 1865.

One explanation for the difference in the rates of persistence of male and female names can be found in the shifts in the frequency of some categories of names among slaves. I have classified names as biblical, classical, common English, Spanish, or French in origin; as place-names, day-names, or African names.<sup>22</sup> Biblical names that were in common English usage were classified as English names, thus defining the biblical category narrowly and including only names that were clearly biblical in origin. Both the African day-names and their Anglicized translations, such as Monday and Friday, have been categorized together, and the day-name category was expanded to include names that were used because they linked the child with an event that occurred at its birth. For this reason, months of the year, seasons and holiday names, such as Christmas and Easter, have been designated as day-names.

<sup>22</sup> George R. Stewart, *American Given Names: Their Origin and History in the Context of the English Language* (New York, 1979), 29–31.



Most striking is the increased use of biblical names for male children of slaves. During the first hundred years, 1720 to 1819, biblical names were chosen for between 20 and 25 percent of male children. In the same period, biblical names accounted for less than 20 percent of all female names. After 1820, the number of biblical names for boys increased dramatically, accounting for four out of ten names. No comparable increase in the use of biblical names for females appears during the nineteenth century.<sup>23</sup>

The gender difference in the use of biblical names can be seen in the names chosen by John Ball, Jr., in 1805 and 1806 for two groups of newly purchased Africans. The six male slaves were all namesakes of figures from the Old Testament, including Nathan, Ishmael, and two sets of brothers, Moses and Aaron, Israel and Esau. The seven female slaves received names derived from a variety of sources—Roxena, Juno, Judy, Tenah, Pallas, Bobbet (presumably, Babette), and Molly.<sup>24</sup>

By the nineteenth century, place-names and day-names, two types of names that were used by slaves but did not appear among the white population, declined in use among the Ball slaves. Place-names such as Windsor, London, Glasgow, and Dublin were common among the first slaves born on the Ball plantations and accounted for about one in four names given to males between 1720 and 1740. Most often, place-names reflected the English and Irish roots of the slaveowners. When the names persisted in use—they constituted about 10 percent of all male names after 1740—they were usually transmitted as kin names. Place-names were almost exclusively male, with only Cuba (possibly the African name Cooba) appearing among females.

Day-names in all their forms also declined in popularity and did not account for more than 5 percent of all male names after 1800. They were rarely used for female slaves. Most day-names, as well as names for holidays, months, and seasons, entered the pool of names as actual day-names but were continued in usage as kin names. Eleven of the fourteen children (78.6 percent) who received Anglicized day-names during the eighteenth century were given names appropriate for their day of birth. By the nineteenth century, the percentage of day-names that accurately recorded the day, month, season, or holiday of a child's birth had declined to fourteen of thirty-five (40 percent). The decline suggests that, although day-naming persisted into the nineteenth century among the Ball slaves, more often than not, the name was selected for another reason, frequently, as in the case of Christmas and Easter, as a kin name.

African day-names that survived in usage in the African form rarely retained the meaning associated with the day. Cuffee ("born on a Friday") was a popular name for male children on the Ball plantations, appearing eight times. However, it was used only once for a boy born on a Friday. Mimba, by African naming rules a name bestowed on girls born on a Saturday, was also a common name, but it was used only two of eight times for a girl born on Saturday, a weak pattern at best,

<sup>23</sup> Inscoc, "Carolina Slave Names," 541–46.

<sup>24</sup> John Ball Plantation Books, Southern Historical Collection, Chapel Hill.

TABLE 5

Percentage of Names Given to Slave Children at Birth on the Ball Plantations, 1720-1865  
by Gender, Classification, and Cohort

Name type	1720-39 N = 32	1740-59 N = 42	1760-79 N = 73	1780-99 N = 86	1800-19 N = 285	1820-39 N = 402	1840-65 N = 243	1720-1865 N = 1163
MALES								
Biblical	18.6	23.8	26.0	20.1	26.3	43.3	39.5	34.2
Classical	15.6	23.8	15.1	22.1	10.5	9.5	10.0	11.8
English	12.5	9.5	13.7	15.1	17.9	24.6	21.0	19.9
Spanish	—	2.4	—	3.5	2.0	0.2	2.1	1.5
French	—	2.4	2.7	3.5	1.8	2.5	1.2	2.1
Place-name	25.0	4.8	8.2	9.3	6.0	3.5	7.0	6.2
Day-name	6.3	9.5	6.8	10.4	4.6	3.7	2.5	4.6
African	—	4.8	—	—	1.1	0.4	0.4	0.7
Unclassified	21.9	19.0	27.4	15.1	29.1	17.2	16.5	18.9
FEMALES	N = 26	N = 42	N = 90	N = 77	N = 273	N = 396	N = 277	N = 1181
Biblical	19.2	11.9	16.7	16.9	15.8	21.0	15.2	17.4
Classical	7.7	14.3	5.6	5.2	10.6	5.6	10.8	8.3
English	38.5	40.5	44.4	40.3	39.6	41.7	44.8	41.9
Spanish	26.9	4.8	10.0	7.8	8.8	7.1	14.4	9.8
Floral	—	4.8	5.6	1.3	3.7	1.3	2.9	2.6
Day-name	—	2.4	1.1	2.6	2.2	1.8	—	1.4
African	2.7	4.8	6.7	9.1	7.3	6.8	5.8	6.7
Unclassified	2.7	16.7	10.0	16.9	12.1	14.9	6.1	11.8

since even a random use of the name would result in an appropriate day-name use in one out of seven instances.

Peter Wood's suggestion that some common slave names survived because they closely resembled the sound of more traditional African day-names is not supported by the evidence of the Ball plantations.<sup>25</sup> If these names did persist because of their sound, they rarely retained the African meaning Wood implied. Jack, a name that Wood argued may have persisted among slaves because it was derived from Quaco ("boy born on a Wednesday") was given to a child born on that day in only two of twelve instances. The name Phoebe, which Wood maintained was derived from Phiba, for a girl born on a Friday, was used eight times but never for a girl born on that day.

Classical names declined in use after 1800 as well. Like place-names and day-names, classical names were rarely found among the slaveowners. These names were associated almost exclusively with slaves. Prior to 1800, classical names accounted for about 20 percent of names given to male slaves born on the Ball plantations. During the nineteenth century, the share of classical names declined to about 10 percent. The gender difference in the frequency of classical names illustrates how these names came into use. As elsewhere in the colonial South, favorite names included Pompey, Cato, and Caesar, indicating that the Balls were well acquainted with *Plutarch's Lives* and turned to the classics and ancient history as a means of creating a diverse pool of slave names. Female characters are rare in these sources and provided few potential namesakes for slaves.<sup>26</sup>

Other slaves born in the eighteenth century received names that reflect the literary tastes of their owners. The plays of Shakespeare provided the names Cordelia, from *King Lear*, and possibly Celia, from *As You Like It*, as well as for one child, Romeo. Evidence suggests that the Balls also selected characters from popular contemporary fiction as namesakes for their slaves. The list of slaves in 1784 at Limerick plantation records an adult male slave named Gil Blas, whose name was continued into the next generation by his son. Since it is unlikely that the slaves had access to Alain René Le Sage's *History and Adventures of Gil Blas of Santillane*, the probable source for the name was the owner's library.<sup>27</sup>

THE TRANSITION FROM SLAVE NAMES TO NAMES MORE WIDELY USED by other populations appears in a comparison of the leading names for the Ball slaves with those of their owners and the most common names in Hingham in Massachusetts and Middlesex County in Virginia.<sup>28</sup> The leading names for male and female children born on the Ball plantations for 1720 to 1799, 1800 to 1865, and the entire period are reported in Tables 6 and 7.<sup>29</sup> Male names on the Ball plantations

<sup>25</sup> Wood, *Black Majority*, 182–83; Joyner, *Down by the Riverside*, 218–19.

<sup>26</sup> "Catalogue of Books at Marshland Farm Belonging to John Ball," Ball Family Papers, Perkins Library, Duke University, Durham, N.C.

<sup>27</sup> *Ibid.*

<sup>28</sup> Smith, "Child-Naming Practices"; and Rutman and Rutman, *A Place in Time: Explicatus*, 83–106.

<sup>29</sup> Three features of these distributions should be noted. First, as a result of the large size of the pool of names and the relatively small number of cases, many of the rankings are shared by two or more

between 1720 and 1799 bear only a partial resemblance to the most common names among blacks and whites in the Middlesex and Hingham populations of the eighteenth century, reflecting both the distinctive quality of black names and perhaps the local character of the slaves and slaveowners. Only six names, Tom, Dick, John (Jack), Jemme (James), Peter, and Charles, rank among the leading fifteen names for both Middlesex slaves and the Ball family slaves. These common English names were also among the leading names for whites in Middlesex. With one exception, the same six names were among the leading names in eighteenth-century Hingham (Smith's 1741 to 1780 parental marriage cohort). Names that were used exclusively among slaves did not overlap the two slave populations. Cupid, Alix, Prince, and Cuffee were popular names only among Ball slaves and were not shared to a large degree by slaves in Middlesex. The choice of "Maurey" and "Maurice" for Ball slaves may be attributed to the French Protestant influence present in the Low Country but not felt in either Middlesex or Hingham.<sup>30</sup> Regional variations in the Euro-American population evidently could affect the names of slaves.

The set of male names for slaves born between 1800 and 1865 on the Ball plantations shares many elements with the leading names in Hingham (Smith's 1781 to 1820 parental marriage cohort). Nine of the most popular fifteen names appear in both populations, which selected the same three leading names, William, John, and Thomas. The two populations also shared the common English names Charles, George, and James and the names of the biblical patriarchs, although "Abraham" is curiously absent from the Hingham list. Only one name that is obviously reserved exclusively for slaves appears among the leading names for Ball slaves—Prince.

Female slaves on the Ball plantations consistently shared a smaller group of names with both the Hingham population and earlier generations of whites and blacks in Middlesex. Prior to 1800, the Ball slaves shared only the names Mary, Betty, Jenny, Hannah, and Beck with slaves born and entering Middlesex. For roughly the same period (Smith's 1741 to 1780 parental marriage cohort), they also shared five names with Hingham women (Mary, Betty/Elizabeth, Hannah, Rebecca, and Rachel). The most popular names for female Ball slaves born in the

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names. Second, names used with less frequency and ranked lower than the leading names have an even greater number of tied rankings. These two features render more powerful a statistical approach to the comparative ranking and ordering of the names of limited value. The Rutmans compute Spearman's rho and Pearsonian r as correlation coefficients between each period and the preceding period for which they have naming evidence. This method is problematic in distributions with a large number of tied rankings. The result of such computations is an apparently higher level of correlation. On the uses and misuses of Spearman's rho, see John Mueller, Karl Schuessler, and Herbert Coster, *Statistical Reasoning in Sociology* (Boston, 1977), 260–63. Third, formal names and diminutives that are counted together should be considered related names but not interchangeable in the written record of the slave population maintained by the Balls. Thus, Mary, Maria, Maryanne, and Marion were four individuals who shared a name that was common in root but not used interchangeably. Because slaveowners were interested in maintaining a broad pool of names, they carefully and consistently recorded all variations in the sound of a name as a distinctive name. This is not to say that nicknames were not in use; however, the written record does not indicate this type of variation for individuals.

<sup>30</sup> On the Huguenot influence in South Carolina, see Jon Butler, *The Huguenots in America: A Refugee People in New World Society* (Cambridge, Mass., 1983).

nineteenth century overlapped to a greater extent with those found for Middlesex blacks of the eighteenth century than they did with the white populations of Hingham or Middlesex's residents. Six of the most frequent fourteen names were shared with Hingham, and five of those names with one addition were also in common use among Middlesex's whites. Six names were shared by Middlesex's native-born blacks, and eight of the leading fourteen names can be found among slaves entering eighteenth-century Middlesex County or the Ball slaves born in the nineteenth century. One explanation for this pattern may be that some of the names used by slaves were uniquely identified with slave women. "Binah" may have had West African origins in the names Binta or Beneba, or it may have been derived from the name Subrinagoddard.<sup>31</sup> Binah appears on none of the lists of leading names in Hingham or Middlesex. Hagar, the servant who bore the biblical Abraham's son Ishmael, would be an unlikely choice of name for a white woman.

Despite the shifts in choices of slave names, especially the names of male slaves, the long-term impact of the initially diverse pool of names can be detected well into the nineteenth century. One way to index the comparative size of the pool of names and the frequency with which the most common names were used is to determine the percentage of names concentrated at the top of each cohort. As previously noted, members of the Ball family relied so heavily on family names that more than half of the family's sons received one of four names and half of the daughters received one of six names. Because a high degree of name concentration within a lineage is only logical, a more useful comparison can be made with the Hingham population (Smith's 1781 to 1820 parental marriage cohort). In Hingham, 16.3 percent of the male children were given one of the three leading names, William, John, or Thomas. The same names, which were also the leading names for Ball slaves born after 1800, accounted for a total of 7.1 percent of all male names, or less than half the concentration at the top as seen in Hingham. The twenty most frequent names at Hingham contributed more than half (53.1 percent) of all names given to the community's boys. On the Ball plantations, about one-third (34.8 percent) of the boys received one of the nineteen leading names. Female names on the Ball plantations and at Hingham were more heavily concentrated among leading names. Nearly one in four girls (24.6 percent) of Hingham received one of the three leading names—Mary, Lydia, or Hannah. On the Ball plantations, 10.5 percent of the girls received one of the three leading names—Mary, Elizabeth, or Binah. More than 70 percent of the girls in Hingham as compared to 36.2 percent of the Ball slave girls bore one of the twenty most common names.

Comparison with the leading names for slaves and whites in other populations indicates that male slaves on the Ball plantations, especially after 1800, were beginning to share a common set of names with white populations, especially that of Hingham, in which a strong biblical influence was felt. Female slaves maintained many names that were associated exclusively with their slave status and that set

<sup>31</sup> Wood cites J. L. Dillard in suggesting that the popularity of Binah as a South Carolina slave name may have as its root the African day-name Cubena, for girls born on a Tuesday. See Wood, *Black Majority*, 183; and J. L. Dillard, *Black English* (New York, 1972), 129–30.

**TABLE 6**  
**Leading Names for Male Slave Children Born on the Ball Plantations, 1720-1865**

Leading Names for Male Slave Children Born on the Ball Plantations, 1720-1863												Cohort In Which Formal Name First Appeared
1720-1799 (N = 233)				1800-1865 (N = 930)				1720-1865 (N = 1163)				
Rank	Name	N	Percent	Rank	Name	N	Percent	Rank	Name	N	Percent	
1st	Cupid	6	2.6	1st	William-Billy	27	2.9	1st	William-Billy	30	2.6	1800-19
2d	Alix	5	2.1	2d	John-Johnny	22	2.4	2d	John-Johnny	27	2.3	1780-99
2d	Charles-Charley	5	2.1	3d	Nathan-Nat	21	2.3	3d	Thomas-Tom	26	2.2	1800-19
2d	Cuffee	5	2.1	3d	Robert-Bob	21	2.3	4th	Nathan-Nat	24	2.1	1800-19
2d	Dick-Dicky	5	2.1	3d	Thomas-Tom	21	2.3	5th	Robert-Bob	23	2.0	1800-19
2d	John-Johnny	5	2.1	6th	Abraham	19	2.0	6th	Abraham	20	1.7	—
2d	Maurey	5	2.1	7th	Isaac	17	1.8	6th	Charles-Charley	20	1.7	1720-39
2d	Prince	5	2.1	8th	Benjamin-Ben	16	1.7	6th	Jacob	20	1.7	—
2d	Tom	5	2.1	8th	Jacob	16	1.7	6th	Prince	20	1.7	—
10th	David	4	1.7	10th	Charles	15	1.6	10th	Benjamin-Ben	19	1.6	1800-19
10th	Devonshire	4	1.7	10th	Richard-Dicky	15	1.6	11th	Isaac	18	1.5	—
10th	Fortune	4	1.7	10th	George	15	1.6	11th	James-Jimmy-Jim	18	1.5	1760-79
10th	Jacob	4	1.7	10th	Jacky	15	1.6	11th	Maurice-Maurey	18	1.5	1740-59
10th	Jimmy	4	1.7	10th	James-Jimmy	15	1.6	14th	Richard-Dicky-Dick	17	1.5	1800-19
10th	Peter	4	1.7	10th	Prince	15	1.6	14th	George	17	1.5	—
10th	Pino	4	1.7	16th	Joseph	14	1.5	14th	Jack-Jacky	17	1.5	—
10th	Windsor	4	1.7	16th	Samuel-Sam	14	1.5	14th	Peter	17	1.5	—
				18th	Maurice-Maurey	13	1.4	14th	Samuel-Sammy-Sam	17	1.5	1820-39
				18th	Peter	13	1.4	19th	Alexander-Alix	15	1.3	1840-65
Total		83	35.6	Total		324	34.8	Total	Joseph-Josey	398	34.2	1800-19
			Percent				Percent				Percent	
3 Leading Names			6.9				7.5				7.1	
5 Leading Names			11.2				12.0				11.2	
10 Leading Names			21.9				21.0				19.6	



**TABLE 7**  
**Leading Names for Female Slave Children Born on the Ball Plantations, 1720–1865**

1720–1799 ( <i>N</i> = 235)				1800–1865 ( <i>N</i> = 946)				1720–1865 ( <i>N</i> = 1181)				Cohort In Which Formal Name First Appeared
Rank	Name	N	Percent	Rank	Name	N	Percent	Rank	Name	N	Percent	
1st	Mary-Maria	13	5.5	1st	Mary-Maria	38	4.0	1st	Mary-Maria	51	4.3	—
2d	Hannah	8	3.4	2d	Elizabeth-Betty	35	3.7	2d	Elizabeth-Betty	41	3.5	1820–39
3d	Jenny	7	3.0	3d	Binah	30	3.2	3d	Binah	32	2.7	—
4th	Betty-Betsy	6	2.6	4th	Susanna-Sue-Susey	24	2.5	4th	Susanna-Sue-Susey	29	2.5	1800–19
4th	Dolly	6	2.6	5th	Rebecca-Beck-Becky	23	2.4	5th	Rebecca-Beck-Becky	28	2.4	1800–19
6th	Beck-Becky	5	2.1	6th	Judy	21	2.2	6th	Diana-Dye	25	2.1	1760–79
6th	Diana-Dye	5	2.1	7th	Diana-Dye	20	2.1	7th	Judy	23	1.9	—
8th	Amev	4	1.7	8th	Sarah-Sarey	19	2.0	8th	Hannah	21	1.8	—
8th	Celia	4	1.7	9th	Hagar	16	1.7	8th	Sarah-Sarey	21	1.8	1740–59
8th	Clarinda	4	1.7	9th	Nancy	16	1.7	10th	Molly-Molsy	19	1.6	—
8th	Molly	4	1.7	11th	Lucy	15	1.6	10th	Nancy	19	1.6	—
8th	Peggy	4	1.7	11th	Molly-Molsy	15	1.6	10th	Rachel	19	1.6	—
8th	Rachel	4	1.7	11th	Rachel	15	1.6	13th	Hagar	18	1.5	—
8th	Sally	4	1.7	14th	Bella	14	1.5	14th	Jenny	17	1.4	—
8th	Sylvia	4	1.7	15th	Cate	13	1.4	14th	Lucy	17	1.4	—
8th	Tenah	4	1.7	15th	Flora	13	1.4	16th	Catherine-Cate-Catey	16	1.4	1820–39
8th	Tenang	4	1.7	15th	Hannah	13	1.4	16th	Flora	16	1.4	—
				15th	Harriet	13	1.4	16th	Harriet	17	1.4	—
				19th	*			19th	Bella	15	1.3	—
								20th	Bella**			—
Total		90	38.3			353	37.3			443	36.2	
3 Leading Names				Percent				Percent				
5 Leading Names				11.9				10.5				
10 Leading Names				17.1				15.4				
				26.4				24.6				

\* "Lydia" appeared eleven times as a name for a girl born on the Ball plantations between 1800 and 1865. Four names were used ten times; Jenny, Amev, Tenah, and Sylvia. The name Jenny, which ranked third prior to 1800, declined to twentieth (tied with three others for the position).

\*\* Between 1720 and 1865, the names Amev, Sylvia, Tenah, and Lydia were used fourteen times each.

them apart from white populations. Although a weak trend toward biblical and common English names can be detected, the infrequent use of the most popular names suggests a stronger influence. Slaves continued to perpetuate as kin names those names that originated in the existing name system based on owner-selected names for the first and second generations.

BY FAR THE MOST INTRIGUING SHIFT IN THE PATTERN of slave naming is the increased use of biblical names. Although planters' records fail to reveal the precise timing and the process by which the Ball slaves were converted to Christianity, they do reveal that, by the 1830s, a black preacher with his own church was active at Comingtee plantation. Efforts to convert the slaves may have begun as early as the 1780s, when British abolitionists and anti-abolitionists published essays linking the successful reproduction of slave populations in the New World with the conversion of slaves to Christianity and the promotion of stable marriages among slaves. The Balls were well acquainted with these writings. An inventory in 1817 of more than 400 volumes in John Ball, Sr.'s library included both the works of British abolitionist James Ramsey and the pro-slavery writings of Bishop Beilby Porteus.<sup>32</sup> Ramsey's "Essay on the Treatment and Conversion of the African Slaves in the British Sugar Islands," published in 1784, recommended the conversion of the slave population as part of a humanitarian program to improve the quality of slave life.<sup>33</sup> Bishop Porteus saw the conversion of slaves as one means of reducing promiscuity and increasing fertility among Caribbean slaves.<sup>34</sup> Whatever the influence of these authors, the demographic records suggest that, by 1780, the Balls were developing a policy of maintaining and increasing their labor force through natural increase by adjusting plantation populations to a more balanced sex ratio.<sup>35</sup>

The timing of the rise in the use of biblical names for male slaves on the Ball plantations is consistent with the conversion of slaves during the first two decades of the nineteenth century. In fact, the biblical names that slaves chose indicate not a mere acquaintance with but a clear understanding of a set of Bible stories, primarily from the Old Testament. The evidence is strong enough to show that these stories were the core of the oral religious instruction given the slaves. The lessons provided new names, a set of character values, and a model of the structure of a patriarchal society.

To analyze most effectively the use of biblical names, I have grouped them by Bible stories. Families of the Old Testament patriarchs, Moses and the Israelites, King David, Adam and Eve, Samson and Delilah, Lazarus, Mary, and Martha

<sup>32</sup> "Catalogue of Books at Marshland Farm."

<sup>33</sup> James Ramsey, "An Essay on the Treatment and Conversion of the African Slaves in the British Sugar Islands" (London, 1784); see Folarin Shyllon, *James Ramsey: The Unknown Abolitionist* (Edinburgh, 1977).

<sup>34</sup> Beilby Porteus, "A Letter to the Governors, Legislatures, and Proprietors of Plantations in the British West India Islands" (London, 1808).

<sup>35</sup> In 1784, only Comingtee had a gross imbalance between the sexes. At Kensington, also owned by Elias Ball, the sex ratio among adults was 91 males per 100 females. Cody, "Slave Demography and Family Formation," 51–54.

provided names and examples of vice and virtue. Names associated with the patriarchs were most popular: Abraham, Isaac, Jacob, and Joseph. The story of Abraham mentions his wife, Sarah, their son, Isaac, Sarah's Egyptian servant, Hagar, and the son of Abraham and Hagar, Ishmael. Isaac's stories name his wife, Rebecca, and twin sons, Esau and Jacob (later, Israel). Jacob's stories detail his marriages to Laban's daughters, Leah and Rachel, and the births of his daughter, Dinah, and his twelve sons, including Joseph and Benjamin.

In selecting biblical names for their children, the slaves avoided some names, especially those of figures who exhibited deep flaws of character or appearance. From the story of Samson, slave parents chose the name of the hero for their sons but never the name Delilah for their daughters. From the stories of King David, slaves broke the continuity of the line of leadership—Hannah, Samuel, Saul, David, and Solomon—by omitting Saul. For those who heard his story recounted, Saul's jealousy, his erratic behavior, and suicide on the field of battle probably outweighed his positive qualities. Slaves avoided other namesakes from the King David stories. There was no Absalom on the Ball plantations. No slave parents used as a biblical namesake this disloyal son who rose in rebellion against his father. Nor did slave parents name a daughter for the adultress Bathsheba, though Uriah, the wronged husband, provided the name for one Comingtee slave.

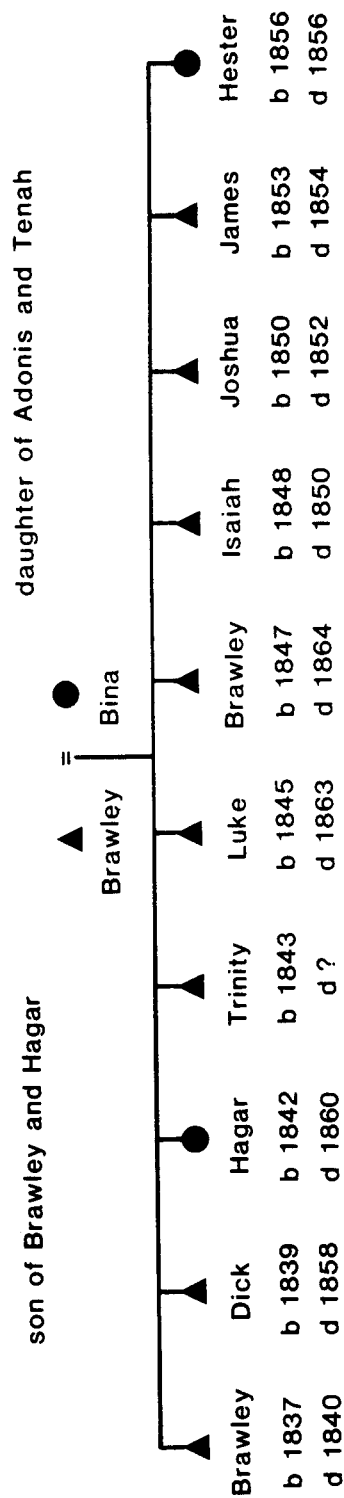
Biblical stories supplied not only names for slaves but also a model of a system of kinship that emphasized the paternal line, as in the stories of the Old Testament. The link between conversion to Christianity and a paternal preference in naming is evident in the family of Preacher Brawley and his wife, Bina. Preacher Brawley worked as a hog minder at Comingtee. He built and preached in a "rough little clap-board church." According to his owners, he "professed and retained a certain amount of faithfulness to his master, but it was of a limited kind. As a preacher, he had considerable influence on the plantation; and, though tricky, was of great use, after the war, in inducing the hands to sign contracts."<sup>36</sup>

Preacher Brawley and Bina preferred biblical characters and paternal kin as namesakes for their children. Two sons, Trinity and Luke, were given names of biblical origin and new to the Ball slave population. Brawley, the first-born son, shared the name of both his father and paternal grandfather. After the death of the first young Brawley, Preacher Brawley and Bina reused the name for their fifth son, attempting to ensure the name replication of the paternal line into the next generation. They named their first daughter Hagar, for her paternal grandmother, reflecting a preference for paternal kin naming. No daughter shared the name of her mother or maternal grandmother, and, despite the birth of eight grandsons, Bina's father, Adonis, did not have a child named for him.

The preference for biblical names, especially those of the Old Testament patriarchs, brought a potential conflict with one of the rules that governed the selection of names. Ball family slaves avoided or were forbidden the use of their owner's name during his lifetime, as well as family names that were unique to the owner's lineage. This prohibition would be logical, given the emphasis the Balls

<sup>36</sup> Deas, *Recollections of Comingtee*, 166–67.

FIGURE C  
CHILDREN OF PREACHER BRAWLEY AND BINA



placed on protecting and perpetuating family names. For example, no slave at any of the plantations received the name of the family patriarch and first immigrant, Elias. The use of the name Isaac is more instructive. The first appearance of Isaac as a slave name occurred in 1778, two years after Elias's son Isaac died. The Ball family reused "Isaac" in 1785 and again in 1818. During the period 1780 to 1825, Ball slaves avoided the name. After both Isaac Balls died in 1824 and 1825, respectively, the slaves began to use the name for their children, naming ten boys Isaac over the next decade. Two slave families used Isaac as a necronymic sibling name. Although, for more than forty-five years, the slaves avoided using the name Isaac, they increasingly used biblical names, especially the names of the Old Testament patriarchs and the women associated with them. Names from the stories of Isaac run through the records: Abraham (six children), Sarah (ten), and Hagar (ten); Rebecca, Isaac's wife (thirteen), although "Rebecca" was popular prior to the transition to biblical naming. The biblical Isaac's sons, Esau and Jacob, were also sources of names. Jacob was the name for fourteen children, while the less fortunate Esau, only one.

The stories of the Old Testament place great emphasis on paternal kin reckoning and may have influenced a shift toward a paternal preference in the selection of kin names among slaves. In selecting kin names for their children, the Ball family slaves made a transition after 1800 from a system that favored the grandfather as a namesake to one that emphasized ties to the father. In addition, they preferred paternal over maternal kin in selecting family names, more nearly replicating the naming practices of their owners.

A look at kin naming among the Ball slaves before and after 1800 suggests kin naming never achieved the intensity that it did among members of the Ball family. Before 1800, when they selected a kin name, the Ball slaves more often turned to a grandfather as the namesake than to the father and were equally likely to select the paternal and maternal grandfathers. In only 15.2 percent of the thirty-three families in which the first child was born prior to 1800 did the slaves name a son for his father. In the thirteen fully reconstructed families, only 15.4 percent transmitted the name of a father to his son. More than 60 percent of the slave families succeeded in reusing the paternal grandfather's name for a son. Similarly, 60 percent of the families transmitted the name of the maternal grandfather. This pattern, albeit based on a small number of cases, suggests that the first generations of American-born slaves selected as kin names for their children names that linked the child to a more distant and, in some cases, African past, without preference for patrilineality.

When the slave families named daughters, they preferred grandmothers as namesakes. Only three of the thirty-three families (9.1 percent) used the mother as a namesake. Among the seventeen fully reconstructed families, only 11.8 percent transmitted the name of the mother to her daughter. Furthermore, slaves rarely used the paternal grandmother's name, preferring by far the mother's mother (in 70.6 percent of the families).

Slaves revealed their naming priorities when they named their first and second children. Most significantly, about 60 percent of the first and second sons and more

TABLE 8

## Percentage of Ball Slaves Sharing a Kin Name by Gender, Family Size, and Kin Connection, 1720–1799 and 1800–1865

	Sons						Daughters						
	1-2			3 or more			1-2			3 or more			
	N	Percent	N	Percent	N	Percent	N	Percent	N	Percent	N	Percent	
<b>1720-99</b>													
Father	14	7.1	19	21.1	33	15.2	Mother	12	0	21	14.3	33	9.1
(fully reconstructed families*)							(fully reconstructed families)						
Father	4	0	9	22.2	13	15.4	Mother	6	0	11	18.2	17	11.8
Paternal Grandfather (FAFA)	4	75.0	9	55.5	13	61.2	Paternal Grandmother (FAMO)	6	0	11	9.1	17	5.9
Maternal Grandfather (MOFA)	4	25.0	9	77.7	13	61.2	Maternal Grandmother (MOMO)	6	0	11	81.8	17	70.6
<b>1800-65</b>													
Father	57	7.0	76	42.1	133	27.1	Mother	49	4.1	82	22.0	131	15.3
(fully reconstructed families)							(fully reconstructed families)						
Father	11	9.1	27	66.7	38	50.0	Mother	12	0	28	17.9	40	12.5
Paternal Grandfather (FAFA)	11	9.1	27	48.1	38	36.8	Paternal Grandmother (FAMO)	12	33.0	28	39.3	40	37.5
Maternal Grandfather (MOFA)	11	18.2	27	7.4	38	10.5	Maternal Grandmother (MOMO)	12	16.7	28	21.4	40	20.0

\* Includes only families in which all four grandparents are known.



## TABLE 9

**Percentage of First and Second-born Ball Slave Children Sharing a Kin or Biblical Name  
by Gender, Family Size, and Kin Connection, 1720–1799 and 1800–1865  
(Includes only fully reconstructed families)**

Percentage of First and Second Sons Sharing Kin Names						Percentage of First and Second Daughters Sharing Kin Names							
	N	Father	FAFA	Both	MOFA	Neither Parent nor Grandparent		N	Mother	MOMO	Both	FAMO	Neither Parent nor Grandparent
1720–99						1720–99							
1st Son	13	8.7	15.4	0	15.4	61.5		17	0	29.4	0	0	70.6
2d Son	12	0	8.3	0	33.3	58.3		14	0	21.4	0	0	78.6
1800–65						1800–65							
1st Son	38	21.1	15.8	7.9	5.3	65.0		40	0	5.0	0	22.5	72.5
2d Son	35	17.1	11.4	2.9	0	74.4		35	5.7	8.6	0	8.6	77.1
Percentage of First and Second Sons Receiving Biblical Names						Percentage of First and Second Daughters Receiving Biblical Names							
	N		Biblical		Biblical Non-kin*		N		Biblical		Biblical Non-kin		
1720–99						1720–99							
1st Son	13		15.4		15.4	1st Daughter	17		17.6		17.6		
2d Son	12		25.0		16.7	2d Daughter	14		14.3		14.3		
1800–65						1800–65							
1st Son	38		39.5		26.3	1st Daughter	40		35.0		25.0		
2d Son	35		24.3		22.9	2d Daughter	35		25.7		17.1		

\*Biblical names were categorized as non-kin if they were not shared by parent or grandparent.

than 70 percent of the first and second daughters did not share either the name of parent or grandparent. Kin names, then, were perpetuated by subsequent children. One explanation for this pattern could be a necronymic naming system extended among a broadly defined kin.<sup>37</sup> The daughters of Windsor and Angola Ame appear to have practiced a system of kin replacement during the eighteenth century. The mortality evidence for slaves born on the Ball plantations during the eighteenth century is, unfortunately, too weak to test this hypothesis.

After 1800, the Ball slaves altered these naming patterns to emphasize paternal ties, adopting the naming preferences of the owners. Among the 133 slave families that began after 1800, 27.1 percent named a son for his father. Families with three or more sons transmitted the father's name to the next generation for 42.1 percent of the families. In the thirty-eight fully reconstituted families, the pattern is stronger, with half of all families and two-thirds of the families with three or more sons reusing the father's name in the next generation. The paternal grandfather was also a common namesake for slave sons; the maternal grandfather was, however, rarely chosen. For all families, 36.8 percent succeeded in naming a son for his paternal grandfather, while only 10.5 percent perpetuated the name of the mother's father. This pattern suggests that a transition was occurring from a naming system favoring grandfathers' names to one favoring fathers' names and, within the selection of a grandfather's name, a clear preference to replicate the paternal line.

In naming daughters, the Ball slaves persisted in a pattern favoring the name of the grandmother over the name of the mother. Only 15.3 percent of the 131 families in which both parents were known and 12.5 percent of the fully reconstructed families named a daughter for her mother. They perpetuated both the paternal and maternal grandmothers' names, with more than one in three (37.5 percent) of the families naming a daughter for her father's mother. One in five families used the name of the mother's mother. The preference for the paternal grandmother as a namesake is a reversal of the trend discerned for the eighteenth century favoring the maternal line and suggests that confirming ties to the paternal lineage grew in importance during the nineteenth century.

One indication of the strength of commitment to kin naming is the relatively low percentage of families with only one or two sons or daughters who used the name of a parent or grandparent for a child. Because no couple could predict the number of children of each gender they would produce, the selection of names for their first children reflect their priorities. In the nineteenth century as in the eighteenth, Ball slaves named only about three in ten first and second sons and daughters for kin. There is some indication that, in selecting kin names, the Ball slaves may have extended back beyond grandparents to confirm ties to more distant kin. The children of Pompey and Peggy are a case in point. Of the five children who received a kin name on the paternal side, four shared the name of

<sup>37</sup> Cody, "Naming, Kinship and Estate Dispersal," 198–203. Slaves on the Gaillard plantation seem to have used the repetition of kin names to link a child with deceased kin or a family member who had been sold or removed from the plantations.

kin extending back more than two generations. Systematic evidence of this depth of kin recognition cannot be obtained from the Ball records, but the example suggests both the depth and breadth of slave kin naming. As slaves began to prefer paternal kin, at least some began to subordinate kin naming to biblical naming. Such slave couples delayed the use of parental and grandparental names for their children. In families begun after 1800, slave parents named almost as many first and second sons and daughters for non-kin biblical namesakes as for a parent or grandparent.

Previous studies of slave-naming practices reveal one pattern common to the slaveowners that was avoided by slaves—the naming of another child in a nuclear family to replace a deceased sibling. Although the Ball family used necronyms, they took advantage of only about 20 percent of the opportunities presented. This proportion does not suggest an avoidance of the practice but indicates some reluctance to reuse the name of a deceased child. Slaves on the Ball plantations also engaged in necronymic sibling naming. Thirty-six families were involved in the practice and a total of thirty-eight children shared the name of a deceased sibling. As was the case with the Ball family, this represents about a 20 percent use of opportunities to name a child for a dead sibling. It should be noted, however, a family-by-family examination of slaves who practiced necronymic naming reveals that, in thirty of the families, at least one parent in each family was on the “blue list,” that is, was a skilled slave who received a higher quality cloth when allocations were made. Skilled slaves such as drivers, carpenters, poultry keepers, and domestic servants were either more fully acculturated to the naming practices of their owners than were field hands or they experienced greater owner interference when selecting a name for a child.

ONCE AFRICANS ARRIVED IN THE NEW WORLD, they began the process of transforming their African culture into an Afro-American one. Although they were unable to reproduce many of the artifacts of their African slave systems, they preserved the hidden structures by which a society functions. Their values as well as some of their African practices could be retained in the names they selected for their children. The naming practices of the Ball family slaves suggests that the system of slave naming evolved from one based on owner selection, with perhaps some slave participation, to one in which the slaves chose the names of their own children. In this process, the slaves turned first to the preservation of kin names. The first generation of American-born slaves used the names of their children to connect grandparent with grandchild, second generation American-born to African-born. By drawing kin connections to a wide network of family, slaves developed a kin-naming system that was distinctive from that of their owners.

This distinction was to be short-lived as slaves were exposed to other forces that influenced the names they selected and the naming practices they chose. Like other slave populations, the slaves on the Ball plantations were exposed to the teachings of the Bible, which provided not only names but also a model of a patrilineal society. As they turned to biblical names with greater frequency, slaves revealed

a preference for some characters, a distaste for others, and structured a kin system that emphasized the connections between father and son and among broader relations of paternal kin. In transforming their naming system from one that was based on bilateral kin reckoning and that sought significant ties between the present and more distant past, slaves changed their naming practices to resemble more closely those of their owners.

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# The Making of an American Prophet: Emerson, His Audiences, and the Rise of the Culture Industry in Nineteenth-Century America

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MARY KUPIEC CAYTON

AT ONE TIME, HISTORIANS THOUGHT, INTELLECTUALS REPRESENTED the highest and best a culture had to offer. Twenty years ago, in an era of works about prominent intellectuals, the story of one person's life could provide an entry into the tensions experienced on a lesser scale by the culture as a whole. As the history of political cultures has given way to the practice of social history and the study of ideas to the history of ideologies, historians are less clear about what to make of highly visible intellectuals. Many studies of the lives of the "common people" and the working class have illuminated class tensions and the dynamics of social change. But historians working in the wake of the new social history seem ambivalent about what the lives of prominent persons can tell us, except as mere windows into a small sector of highly elite culture, of whose nature we seem already to be well aware.<sup>1</sup>

In part, the historiographical confusion about the role of intellectuals may stem from a failure of method. What are historians to make of the notions of "ideas" or "thought" themselves? Literary theorists have hotly debated the issue over the

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<sup>1</sup> For commentary regarding the contemporary difficulties inherent in the practice of intellectual history, see John Higham and Paul K. Conkin, eds., *New Directions in American Intellectual History* (Baltimore, Md., 1979). Works written in the consensual tradition in which the lives of prominent individuals reflect the tensions within culture as a whole include John William Ward, *Andrew Jackson: Symbol for an Age* (New York, 1955); and John L. Thomas, *The Liberator: William Lloyd Garrison, A Biography* (Boston, 1963). Contemporary works such as Kathryn Kish Sklar's *Catharine Beecher: A Study in American Domesticity* (New Haven, Conn., 1973); and Lewis Perry's *Childhood, Marriage, and Reform: Henry Clarke Wright, 1797–1870* (Chicago, 1980), use the lives of reformers prominent within certain cultural circles to illumine the lives of those groups. More common today are lives of "ordinary people," whose experience suggests that of others within their cultural or class milieu and which serve as "case studies." See, for example, Alfred E. Young, "George Robert Twelves Hewes (1742–1840): A Boston Shoemaker and the Memory of the American Revolution," *William and Mary Quarterly*, 3d ser., 38 (1981): 561–623; Paul E. Johnson, "The Modernization of Mayo Greenleaf Patch: Land, Family, and Marginality in New England, 1766–1818," *New England Quarterly*, 55 (1982): 488–516; and Christopher M. Jedrev, *The World of John Cleaveland: Family and Community in Eighteenth-Century New England* (New York, 1979).

course of the last decade and a half (although they speak of “texts” and “discourse”), and the general proliferation of theory in response to the question points to an epistemological crisis whose implications extend beyond disciplinary boundaries. What is meaning? Who makes it? How is it made? In discussions of ideologies and *mentalités*, historians have focused on the ways in which certain structures—economic, political, racial, gender-specific—give rise to cultural meanings, but we remain without an adequate way of thinking about the roles individuals play in transforming those cultural meanings.<sup>2</sup>

Discussions of literary theory and the production of texts suggest two approaches that may prove helpful in reassessing the role of intellectuals in the production of cultural meanings. The first, reception theory, views the text as a process of signification constructed by the reader (or groups of readers). That is, the production of meaning inheres in the interaction of reader and text, not solely in the intent of the writer or the structure of the text. The implications for the intellectual historian are clear: cultural meanings emerge as specific audiences interpret the utterances of those individuals in society who function as intellectuals. The intellectual discourse that emerges from a given culture is made not by “great men” who transcend the conditions of their age but by communities of listeners who define “discourse conventions” and by the intellectuals to whom they choose to listen. These audiences necessarily filter what they hear through their own material conditions and social experience. A corollary to this audience-oriented notion of intellectual discourse, suggested by the work of Mikhail Bakhtin, concerns the struggle of conflicting groups in specific social conditions within a heterogeneous society to define the nature of symbolically generated meanings. If groups living under different material conditions hear differently, then intellectual discourse consists, first, of those utterances that audiences in a particular time and place and living in specific conditions can hear; second, of the way in which those utterances are heard by those audiences, in contrast perhaps to what a given speaker may have intended; and third, of the contest between conflicting groups of audiences (who hear differently because of different material or social contexts) to imbue the utterance with determinate meaning. In other words, if historians view intellectual discourse as, in large part, a product of audience construction, then the ideas of individual intellectuals *as the audience receives them culturally* become an arena in which historians can examine the conflicts between social groups over the hegemonic making of meaning for that culture.<sup>3</sup>

The case of Ralph Waldo Emerson, one of the most celebrated of American intellectuals, can shed light on the ways in which meanings are made in intellectual

<sup>2</sup> For an excellent summary of the dialogue in the literary community concerning the meaning of texts and the nature of literary discourse, see Terry Eagleton, *Literary Theory: An Introduction* (Minneapolis, Minn., 1983).

<sup>3</sup> The major proponents of reception theory include Wolfgang Iser, *The Act of Reading* (London, 1978); Roman Ingarden, *The Literary Work of Art* (Evanston, Ill., 1973); and Stanley Fish, *Is There a Text In This Class? The Authority of Interpretive Communities* (Cambridge, Mass., 1980). See also Jane P. Tompkins, ed., *Reader-Response Criticism* (Baltimore, Md., 1980). On Bakhtin, see Eagleton's commentary, *Literary Theory*, 116–18; and Michael Holquist, “Answering as Authoring: Mikhail Bakhtin's Trans-Linguistics,” *Critical Inquiry*, 10 (1983): 59–71.



discourse and what those meanings have to do with those people not filling the role of intellectual within the culture. Historians have never known precisely how to categorize Emerson. Perry Miller saw him as the heir and transformer of Edwardian Puritanism. F. O. Matthiessen saw in him the founder of American literary romanticism and termed his the “age of Emerson.” Others (notably Stanley Elkins) have seen him as a prime mover in a generation of reformers, with Transcendentalism being a dangerously uncompromising Emersonian movement for social reform. He was also, according to various scholars, a democratic philosopher, an incipient Darwinist, and a pragmatic mystic.<sup>4</sup> Reception theory suggests that Emerson’s cultural impact may have depended less on what he intended than on what key communities of interpreters made of him.

IF REACTION IN THE POPULAR AND RELIGIOUS PRESS and in the journals of the literary community is any gauge, Emerson attained a limited, local notoriety in his native New England during the 1830s. He was born in Boston in 1803, the son of a prominent Unitarian minister who died young. Prior to 1825, Emerson seems to have viewed himself (to judge by his journals and letters) primarily as a fledgling poet who hoped to make his mark on the world of *belles-lettres*. In need of both money and a socially sanctioned way of indulging his proclivities for philosophizing, he entered upon the study of the ministry, eventually assuming the pastorate of the Second (Unitarian) Church, Boston. He resigned in 1832: his ministerial colleagues and his congregation interpreted the role of the Lord’s Supper celebration in the spiritual life of the church in a way he had come to view as intolerable. After a period of travel, he returned to his ancestral home of Concord, lecturing occasionally, substituting for local ministers, and preaching from time to time in vacant pulpits. Freed from immediate financial pressure by his wife’s legacy, he also spent time during the period 1834–36 reading, thinking, and writing his challenge to the epistemology of the time, *Nature*.

Prior to 1836, Emerson seems to have been viewed by his contemporaries much as one might expect: a somewhat unorthodox clergyman whose eccentricities and devotion to literature were within the bounds of acceptability for the Unitarian ministry. Joel Myerson and Robert Burkholder, in their comprehensive Emerson bibliography, have listed fifteen published works, mainly in the religious press, that speak of or implicitly refer to Emerson during the period 1829–35. Nearly all refer to ecclesiastical, literary, or civic activities that would have been well within the province of a Unitarian minister of the day. With the publication of *Nature*,

<sup>4</sup> Perry Miller, “From Edwards to Emerson,” *New England Quarterly*, 13 (1940): 589–617; F. O. Matthiessen, *American Renaissance: Art and Expression in the Age of Emerson and Whitman* (New York, 1941); Stanley M. Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1976). For a variety of views on Emerson spanning 150 years, see *Critical Essays on Ralph Waldo Emerson*, Robert E. Burkholder and Joel Myerson, eds. (Boston, 1983). Some of the major biographies of Emerson include Ralph L. Rusk, *The Life of Ralph Waldo Emerson* (New York, 1949); Joel Porte, *Representative Man: Ralph Waldo Emerson in His Time* (New York, 1979); Maurice Gonnaud, *Individu et société dans l’œuvre de Ralph Waldo Emerson* (Paris, 1964); Stephen E. Whicher, *Freedom and Fate: An Inner Life of Ralph Waldo Emerson* (Philadelphia, Pa., 1953); Gay Wilson Allen, *Waldo Emerson: A Biography* (New York, 1981); and John McAleer, *Ralph Waldo Emerson: Days of Encounter* (Boston, 1984).

attention to Emerson increased but remained within the elite circles of institutional Unitarianism and its literary adjuncts, the Harvard-dominated literary journals of the Boston area.<sup>5</sup> Part of a culture in which literature still functioned principally as a mode of spiritual discourse, the reviewers of *Nature* analyzed something they named philosophical and aesthetic discourse, but, clearly, they meant to read through these in order to see its religious and moral implications.<sup>6</sup>

Emerson emerged as a recognizable national figure in the decade and a half following the publication of *Nature* because his message shifted from being heard in religious and literary terms to being heard as discourse pertaining to something else. That something else seemed to move beyond the conventions of religious or literary discussion and provide a framework that included both. It would not be entirely accurate to call Emerson's message "secular" in contrast to "religious," since both he and his audiences perceived something spiritual in his utterances. Nor am I willing to use the term "popularization," since the process was not necessarily one of simplification and homogenization of a complex, determinate message for a non-expert audience. Rather, something happened from 1836 to 1850 that made Emerson accessible and appealing to a new audience who, because of its own circumstances, was able to hear him in a new and different way.

In lyceums and Mechanics' Institutes, knowledge that had formerly been defined only as religious, literary, or scientific began to be defined also as practical or pragmatic. The lyceum movement in its early days depended mostly on local speakers who had regional reputations. As Donald Scott has noted, early lyceum speakers were usually people with training and connections in other areas of public performance—law or the ministry, for instance—and their drawing power may have been proportional to the audience's familiarity with their other public roles.<sup>7</sup> Some speakers may also have been known through locally printed and distributed sermons, speeches, essays, or textbooks. Speakers not immediately familiar to the audience were probably arranged for by local ministers, lawyers, and other intellectuals, who tended to be part of networks that, in some cases, crossed regions. Emerson's course on "The Times," in New York, 1842, for instance, was arranged by his brother William, a New York lawyer. The new "popular" audience for the lyceums grew out of preexisting networks of intellectuals who began to be heard in new contexts.<sup>8</sup>

<sup>5</sup> Robert E. Burkholder and Joel Myerson, *Emerson: An Annotated Secondary Bibliography* (Pittsburgh, Pa., 1985).

<sup>6</sup> See Burkholder and Myerson, *Bibliography*, 12–27; and selected reactions to *Nature* in *Emerson's Nature—Origin, Growth, Meaning*, Merton M. Sealts, Jr., and Alfred R. Ferguson, eds. (New York, 1969), 74–110.

<sup>7</sup> Donald M. Scott, "The Popular Lecture and the Creation of a Public in Mid-Nineteenth-Century America," *Journal of American History*, 66 (1980): 791–809. On the lyceum movement, see Carl Bode, *The American Lyceum: Town Meeting of the Mind* (New York, 1956); and David Mead, *Yankee Eloquence in the Middle West: The Ohio Lyceum, 1850–1870* (East Lansing, Mich., 1951).

<sup>8</sup> Until 1835, even publication was predominantly a local matter, with few materials marketed outside the area where the bookseller had printed and bound them. Of the lecturers during the first decade of the lyceum in Salem, Massachusetts, for example, fewer than half resided in Salem itself. The rest included locally prominent politicians, ministers, and college professors, such as William Sullivan, Edward and Alexander H. Everett, and Henry Ware, Jr. See William Charvat, "James T. Fields and the Beginnings of Book Promotion, 1840–1855," *Huntington Library Quarterly*, 8 (1944–45):

The existence of a new popular press, growing in conjunction with a burgeoning commercial economy, eventually provided a vehicle that made these early word-of-mouth connections superfluous and masked the origins of the speakers in the religious or legal communities. At first, in the major publishing centers of Boston and New York, coverage of lecturers in newspapers was minimal, lest summaries of lectures steal the speaker's "product" and render it unusable with other audiences. The only evidence in the Boston *Daily Advertiser* of Emerson's lecture course on "Human Life" in Boston in the winter of 1838 took the form of paid advertisements: two in October announcing a course of "ten or more Lectures" and soliciting subscriptions; and individual announcements printed the day of each lecture, advertising its topic, time, location, and price.<sup>9</sup> This neglect of Emerson was neither unique nor the consequence of his late notoriety over the "Divinity School Address." Wendell Phillips's lecture in 1839 at the Boston Lyceum on "The History of Inventions" was announced with much the same lack of fanfare.<sup>10</sup>

Before long, the situation changed, at least in some newspapers. An article in the Boston *Daily Advertiser*, reprinted from the New York *Evening Post*, commented that other newspapers had responded to the public's desire for press coverage of lectures. The *Post* spoke of "the practice which certain newspapers have recently adopted, of reporting the lecture made before the different societies of the city" and felt compelled to explain why it had not covered lectures.<sup>11</sup> The *Post's* misgivings notwithstanding, a number of New York newspapers and literary periodicals began to afford Emerson significant coverage in the mid-1840s. New York controlled the publishing market, and what New Yorkers wrote about, other parts of the country usually read about. "The eastern papers had said much of Mr. Emerson, and we get an eastern mail every day," wrote a Cincinnati correspondent on Emerson's first visit there in 1850.<sup>12</sup> Perhaps the most important paper to cover lectures was Horace Greeley's New York *Tribune*, said to be "the most influential newspaper in the country."<sup>13</sup> Greeley's coverage of the "isms" of the day, sensational in their own way, sold newspapers and rocketed the *Tribune* to a position of importance in the world of the New York press.<sup>14</sup>

76; and *Historical Sketch of the Salem Lyceum, with a List of the Officers and Lectures since its Formation in 1830* (Salem, Mass., 1879), rpt. in Kenneth Walter Cameron, ed., *The Massachusetts Lyceum during the American Renaissance* (Hartford, Conn., 1969), 15–17.

<sup>9</sup> *Boston Daily Advertiser*, 21, 22, and 26 October 1838.

<sup>10</sup> *Boston Daily Advertiser*, 21 February 1839.

<sup>11</sup> To publish accounts of lectures, or even to describe the lecturer so as possibly to misrepresent him, the *Post* argued, was tantamount to robbing him of his daily bread, because it precluded his right to give the lecture again and to receive remuneration for it. Excerpts from this article are reprinted in the *Boston Daily Advertiser and Patriot*, 7 December 1841.

<sup>12</sup> *Salem Register* (Massachusetts), 3 June 1850, p. 2, rpt. in Kenneth Walter Cameron, ed., *Literary Comment in American Renaissance Newspapers* (Hartford, Conn., 1977), 19.

<sup>13</sup> William Alexander Linn, *Horace Greeley* (New York, 1912), 71.

<sup>14</sup> Emerson met Greeley in 1842; throughout the decade, Greeley published sympathetic reports of what had come to be called "transcendentalism." In 1844, Emerson's stock undoubtedly rose further as Greeley hired Margaret Fuller as his regular literary editor. Her first piece for the *Tribune* was a review of Emerson's *Essays, Second Series*. George Ripley, late of Brook Farm, followed her as the *Tribune's* literary critic in 1849. See Linn, *Horace Greeley*, 71–109; Greeley, *Recollections of a Busy Life* (New York, 1869), 169–91; James Parton, *The Life of Horace Greeley* (Boston, 1855), 219–28; and Glyndon

In Boston, the *Daily Advertiser* wholeheartedly endorsed the New York *Evening Post*'s position and refused to publish accounts of lectures. Perhaps in recognition of Emerson's rising popularity as a lecturer, it nevertheless accorded *Essays, First Series*, themselves print versions of the lectures, a lengthy front-page review.<sup>15</sup> This article treated Emerson as part of a literary community and evaluated *Essays* in literary terms. The review was far from flattering; the writer found Emerson's *Essays* tough, distorted, inharmonious, opaque, ponderous, and labored. Yet he expended time and attention on the book, he explained, because, "from its intellectual tendencies, it may be viewed as the representative of a class of works (chiefly of foreign importations) which have met with some success in 'Young England.'"<sup>16</sup>

The British connection the *Advertiser* refers to provides a second clue to the sources of Emerson's early notice as a public figure in the United States. Although Emerson had enjoyed substantial popularity among a certain group of educated, patrician, Unitarian-bred young men in New England, a transatlantic connection contributed significantly to the furthering of Emerson's reputation as a literary figure. The Boston reviewer who took Emerson to task for *Essays*, for example, began his article by noting that the preface to Emerson's book had been written by Thomas Carlyle. Emerson "is brought before the reading public by one of the 'observed' of the day," the reviewer remarked, "and may thus gain a degree of notice, which, we will venture to affirm, he would not else have attracted."<sup>17</sup> Emerson had done Carlyle the service of overseeing the American publication of Carlyle's works and ensuring that he received royalties for them; Carlyle in turn arranged for the publication of a British edition of 750 copies of the *Essays* and wrote a preface. The anonymous reviewer of the *Advertiser* was responding not to the American edition of Emerson's work but to the British edition, published almost half a year after its American counterpart under the patronage of an established British literary figure. The British edition of Emerson's work proved popular enough to be pirated. Beginning with the British publication of *Essays, First Series*, British periodicals began to review Emerson, frequently pairing his name with that of Carlyle. ("A Yankee pocket edition of Carlyle," some called him.)<sup>18</sup> Although the reviews in Great Britain were far from uniform, Emerson

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G. Van Deusen, *Horace Greeley: Nineteenth-Century Crusader* (Philadelphia, Pa., 1953). Charvat indicated that, during the period of the commercialization of the book trade (1840–55), an author's "literary and social contacts"—and those of the publisher—were instrumental in getting books reviewed in the popular press ("Fields and the Beginnings of Book Promotion," 77–78). Hence the contact of Emerson's friends with the influential world of New York periodicals seems particularly significant. Charvat's *Literary Publishing in America, 1790–1850* (Philadelphia, Pa., 1959) provides a fuller description of the way in which publishing networks in the United States operated during this period.

<sup>15</sup> *Boston Daily Advertiser*, 7 December 1841.

<sup>16</sup> *Boston Daily Advertiser*, 16 December 1841.

<sup>17</sup> *Boston Daily Advertiser*, 16 December 1841.

<sup>18</sup> See, for example, George Gilfillan, "Ralph Waldo Emerson; or, The 'Coming Man,'" *Tait's Magazine*, rpt. in *Littell's Living Age*, 17 (April 1848). This article is in turn reprinted in Kenneth Walter Cameron, ed., *Emerson among His Contemporaries* (Hartford, Conn., 1967), 15–19.

was generally noted, whatever his faults, to be a characteristically American product.<sup>19</sup>

Emerson's lecture tour of Britain in 1847–48 increased his standing as a public figure there, and British periodicals took note of him, for better or worse. The several American periodicals that reprinted British literary gleanings—the *Eclectic* or *Littell's Living Age*, for example—picked up the British literary assessments.<sup>20</sup> Emerson made no money from any of his books until after his celebrated tour of England; *English Traits*, published in 1850, was the first to make a profit. An anecdotal example of the role of British publicity in expanding Emerson's reputation beyond his region appears in the autobiography of Moncure Daniel Conway, the young Virginian who became Emerson's hagiographer and his publicist in Cincinnati. Studying law in Warrenton, Virginia, in December of 1847, Conway stumbled on an article about Emerson with extracts from his essays in *Blackwood's*, the Scottish literary review.<sup>21</sup> Conway traveled to a bookstore in Fredericksburg, where a copy of Emerson's "Arithmetic" was in stock but Emerson's *Essays* unheard of. Ordering a copy and remarking on his new literary find to his cousin John, Conway learned that the increased attention to Emerson had prompted John to write an article about him for the *Richmond Examiner*.<sup>22</sup>

Between a growing notice in the New York press, whose literary editors were often originally from New England, and a reputation in British literary periodicals, which persisted in influencing American literary opinion, Emerson's name was becoming familiar by the late 1840s to a class of readers who kept up with literary affairs. Characterization of him in the British popular press during his 1847–48 lecture tour, however, provided him with an image more readily transferable to popular American audiences. Townsend Scudder's work contains substantial evidence that the British press, in presenting Emerson to audiences of the Mechanics' Institutions, substantially de-emphasized the literary and religious aspects of his discourse in order to portray him as an already highly acclaimed American of prophetic stature. He was a man who spoke directly to the heart, not subject to the ordinary canons of logic. Both literary and religious criticism of Emerson continued to be produced, and, in fact, critics frequently evaluated his writing in either negative or decidedly mixed fashion. Even these appraisals, however, began to betray the influence of the popular press: this man was not to

<sup>19</sup> In addition to Gilfillan's article, see "Emerson," *Blackwood's Magazine*, rpt. in *Eclectic Magazine*, 13 (February 1848): 145–58, in turn rpt. in Cameron, *Emerson among His Contemporaries*, 8–14; "Mr. Emerson's Lectures," *Jerrold's Newspaper*, rpt. in *The Daguerrotype*, 2 (12 August 1848): 467–73, and in Cameron, 20–24; "The Emerson Mania," *The English Review*, 12 (September 1849): 139 and following, rpt. in *The Eclectic Magazine*, 23 (December 1849): 546–53, in *Littell's Living Age*, 25 (6 April 1850): 37–38, and in Cameron, 38–39; "Review of Representative Men," *British Quarterly Review*, 11 (1 May 1850): 281–315, rpt. in *Littell's Living Age*, 26 (6 July 1850): 1–16, and in Cameron, 45–56. On the wide notice taken of Emerson in British periodicals from 1840–50, see William J. Sowder, *Emerson's Impact on the British Isles and Canada* (Charlottesville, Va., 1966), 1–28.

<sup>20</sup> E. Douglas Branch, *The Sentimental Years, 1836–1860: A Social History* (New York, 1934), 111.

<sup>21</sup> The article is almost certainly "Emerson," *Blackwood's Edinburgh Magazine*, 62 (December 1847): 643–57. This was the first article on Emerson published in *Blackwood's*.

<sup>22</sup> Moncure Daniel Conway, *Autobiography, Memories and Experiences*, 2 vols. (London, 1904), 1: 68–70.



be evaluated strictly according to the rules defining literary or theological discussion. Rather, he was to be seen as a radical of some sort, whose message was to be judged according to some new, and as yet unarticulated, rules governing “feeling” and “spirit,” and whose exemplary American-ness was defined as somehow crucial to audience reception of his message.<sup>23</sup>

Although dissenters and young Oxford intellectuals formed part of Emerson’s audience, by far the majority of those who heard him in Mechanics’ Institutions were not mechanics at all, but “clerks, shopkeepers, apprentices, &c. . . professional men, merchants, warehousemen, schoolboys.”<sup>24</sup> The new commercial classes coalesced around Emerson despite the fact that they remained relatively oblivious to his notions of theology, metaphysics, society, and government. What such an audience made of Emerson is a perplexing question. Its primary concern lay neither with literature nor theology. Yet, to understand the making of Emerson as a “popular” intellectual, it is crucial to know the mind of his audience. Between 1840 and 1855, Emerson began to be seen not primarily as a religious or literary figure but as something else, and the coalescence of a bourgeois mercantile audience via the press had much to do with this redefinition of role.

THE IMPORTATION OF EASTERN LECTURERS SUCH AS Emerson in the 1850s marked a new phase of the lyceum and lecturing movement in the midwestern United States. The formation of audiences for these lecturers suggests a good deal about the ways in which the emerging American commercial classes “made” Emerson and gave a cultural imprimatur to particular aspects of his message. Over time, various parts of Emerson’s message became obscured through the sheer inability of his listeners to comprehend them as relevant to their own situations. Other parts became exaggerated, probably beyond anything Emerson ever intended, as a result of the coalescing audience’s ability to fit them into discourse patterns and experiences that it brought to its experience of the speaker. Emerson’s audience had come to a sense of group identity long before his arrival, and hearing him seems to have played a part in heightening its self-consciousness as a group. Before examining in detail how this audience heard his message, it is important to look closely at who his listeners were and the common experience they brought to their interpreting.

Nearly every town had its own lyceum by the mid-1830s. Cincinnati’s lyceum was founded in 1830, Cleveland’s in 1832; the Columbus Reading Room and Institute was organized in 1835, and the one in Indianapolis sometime before that year.<sup>25</sup>

<sup>23</sup> Evidence is both summarized and quoted at length in Townsend Scudder, “Emerson’s British Lecture Tour, 1847–1848,” *American Literature*, 7 (1935): 166–80.

<sup>24</sup> Robert Chambers, “Mechanics’ Institutions,” *Papers for the People* (Philadelphia, Pa., 1851), 3: 197–228, quoted in Scudder, “Emerson’s British Lecture Tour,” 35. For more extensive remarks on the nature of Emerson’s British audiences, see Scudder, 15–36; see also David D. Hall, “The Victorian Connection,” in *Victorian America*, Daniel Walker Howe, ed. (Philadelphia, Pa., 1976), 84.

<sup>25</sup> John J. Rowe, “Cincinnati’s Early Cultural and Educational Enterprises,” *Bulletin of the Historical and Philosophical Society of Ohio*, 8 (1950): 304–06; Elbert Jay Benton, *Cultural Study of an American City: Cleveland*, Part II (Cleveland, Ohio, 1944), 38–39; William Alexander Taylor, *Centennial History of*



These lyceums were part of the national movement begun by Josiah Holbrook in 1829 to promote dissemination of useful information, discussion, and debate. Lyceums began to languish within a decade, however, and were forced to change their form of organization. It was to the second form of this lyceum movement that Emerson was eventually invited to speak, and in the character of these newer organizations lie the clues to the nature of Emerson's audience in the Midwest.

Throughout the young cities of the region, Literary Societies, Young Men's Societies, and Young Men's Mercantile Libraries rapidly displaced lyceums. These new organizations were explicitly established by and for the young mercantile classes of the cities. The urban centers of the newer region contained a disproportionate number of young, unmarried men, most of whom lived in boarding houses. In Chicago in 1850, for example, 94 percent of the male population was under the age of fifty, two-thirds were under thirty, and half ranged from fifteen to twenty-nine years of age.<sup>26</sup> Mostly migrants from rural areas, these young clerks, salesmen, bookkeepers, and banktellers were potentially cut off from the influences of family, friends, and church that might have held them in the path of virtue at home. Many writers saw these young men in danger of slipping into vicious habits such as gambling, drinking, theater-going, brothel-visiting, and Sabbath-breaking. A steady stream of advice manuals and tracts poured forth to advise them on the formation and maintenance of character and the path to social acceptability in their new environment.<sup>27</sup> In the cities of the Midwest, the Young Men's Societies provided an alternative gathering-place to taverns and theaters for the young members of the mercantile class. These groups also afforded young men the opportunity to acquire the practical knowledge and debating and speaking skills necessary for social and professional advancement.<sup>28</sup>

The Young Men's Associations, like the lyceums themselves, were anchored in the tenets of the self-culture movement. Self-culture as an ideal originated in urban centers, from the desire of artisans and mechanics to acquire an education in practical and theoretical knowledge of scientific and technical matters. The notion of self-culture quickly took on wider implications: the apostles of the self-culture movement began to advocate the cultivation of an internalized system of morality especially fitted to the newly commercialized portions of the country, particularly, urban areas. Introspective self-examination of conduct would provide highly mobile young men of the urban centers, isolated from traditional institutional

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*Columbus and Franklin County, Ohio*, 2 vols. (Chicago and Columbus, 1909), 1: 241; W. R. Holloway, *Indianapolis: A Historical and Statistical Sketch of the Railroad City* (Indianapolis, Ind., 1870), 50. Mead, *Yankee Eloquence*, provides the fullest account of the growth of the lecture system in Ohio.

<sup>26</sup> Paul Boyer, *Urban Masses and Moral Order in America, 1820–1920* (Cambridge, Mass., 1978), 109.

<sup>27</sup> See Boyer, *Urban Masses*, 108–20; Irvin G. Wyllie, *The Self-Made Man in America: The Myth of Rags to Riches* (New Brunswick, N.J., 1954), 34–54; and Karen Halttunen, *Confidence Men and Painted Women: A Study of Middle-Class Culture in America, 1830–1870* (New Haven, Conn., 1982), 1–32. On the moral dangers inherent in the theater, see Claudia D. Johnson, "That Guilty Third Tier: Prostitution in Nineteenth-Century American Theaters," in Howe, *Victorian America*, 111–20.

<sup>28</sup> William Ellery Channing, in *Self-Culture* (1838) noted the importance of "Utterance"—not only because he considered speaking in public a prime way of improving one's intellect but also because "to have intercourse with respectable people we must speak their language." He noted that social rank and social advancement depended on this fluency. See *Self-Culture* (rpt. edn., New York, 1969), 27–28.

bolsters of morality, the means for maintaining character in a disorienting environment.<sup>29</sup> Within the philosophy, however, a tension existed: the young man had to be self-reliant and independent of external influences but only so that he could remain true to a collective standard of morality in time of trial. "Self-culture" was, in short, an articulation of the *process* whereby moral character might be maintained. The "culture" that succeeded it—and that Emerson's lecturing did its part to promote—focused on the *definition* of collective standards of morality and acceptable behavior.<sup>30</sup>

The self-culture movement in the Midwest was intimately linked to city boosters and businesspeople, those who had the greatest interest in maintaining moral order among the young male migrants to the city. In the Midwest, the gradual commercial development of eastern cities was compressed into a few years, as cities rapidly appeared out of the prairie. As a result of this rapid economic development, the midwestern merchants played a larger part in civic affairs than their counterparts in the East. Because the merchant—not the minister, the lawyer, the politician, or the college professor—was the representative civic figure, the culture movement in the Midwest was, almost from its inception, dominated by the merchant classes.<sup>31</sup> Over and over in their official biographies, successful merchants had their good fortune attributed to self-education and self-culture.<sup>32</sup> It is

<sup>29</sup> Wyllie, *Self-Made Man*, 21–54 and 94–115; and John G. Cawelti, *Apostles of the Self-Made Man* (Chicago, 1965), 39–98.

<sup>30</sup> On character as a defense of virtue against the corrupt, see Halttunen, *Confidence Men and Painted Women*, 1–55.

<sup>31</sup> This is not to suggest that the mercantile classes of eastern cities were not also influential in the establishment of its cultural institutions. The leisure and professional classes nevertheless seem to have been more strongly represented there than in the West. See Ronald Story, "Class and Culture in Boston: The Athenaeum, 1807–1860," *American Quarterly*, 27 (1975): 178–99. An example of mercantile leadership in cultural affairs that I take to be fairly typical in the Midwest was the organization of the St. Louis Mercantile Library Association, the organization that sponsored Emerson's visit in 1852. It was established and funded by merchants and businessmen. Convinced by mercantile journals such as *Hunt's Merchant Magazine* that their young clerks needed to be educated beyond practical matters of business to do their jobs well, merchants took pride in the "inestimable value" that the new association would offer "the young men connected with commerce" by sponsoring lectures and discussions as well as providing books. *Missouri Reporter* (St. Louis), 6 January 1846. The typical founder of the association tended to be an older and established merchant involved in business dealings with the New York financial market and with markets outside St. Louis. He had probably migrated to St. Louis in his youth in search of "a wider field of enterprise," as the stock phrase for the sort of ambition that was positively evaluated went. He looked on himself as a self-made man whose thirst for self-culture led to his success. A canny businessman as well as a morally responsible employer, he took personal responsibility for promoting the reputation of his city as a cultural center. See Brad Luckingham, "A Note on the Significance of the Merchant in the Development of St. Louis Society as Expressed in the Philosophy of the Mercantile Library Association, 1846–1854," *Missouri Historical Review*, 57 (1963): 184–98.

<sup>32</sup> For the St. Louis case as one example, see Richard Edwards and M. Hopewell, *Edward's Great West and Her Commercial Metropolis, Embracing a General View of the West, and a Complete History of St. Louis, from the Landing of Ligueste, in 1764, to the Present Time* (St. Louis, Mo., 1860), 389, which lists thirty-six individuals instrumental in the establishment of the Mercantile Library Association. Of these I have been able to locate biographical information on fourteen. The generalization here is based on the careers of Henry D. Bacon, Robert Barth, William H. Belcher, Taylor Blow, George Knight Budd, John B. Carson, A. B. Chambers, Wayman Crow, Luther M. Kennett, James H. Lucas, John S. McCune, John Simonds, Edward Walsh, and James E. Yeatman. For biographical information on these individuals, see J. Thomas Scharf, *History of Saint Louis City and County, from the Earliest Period to the Present Day*, 2 vols. (Philadelphia, Pa., 1883), 1: 552, 680–81, 688, 871–73; 2: 1162–63, 1245; Charles van Ravenswaay, "Years of Turmoil, Years of Growth: St. Louis in the 1850s," *Missouri Historical Society*

not surprising that they dominated the foundation of a new lecture movement in the Midwest designed to inculcate certain moral values in their protégés. When Emerson came to Pittsburgh in 1851, merchants closed their shops early so that young clerks could go to hear him.<sup>33</sup> The sorts of messages he and other eastern lecturers brought to the platform fit the aims of a mercantile version of the self-culture ideal. In it, recommended activities and ways of thinking led not only to improvement of character but directly to business success.<sup>34</sup>

While sponsoring self-culture activities such as debating, public speaking, and literary study, the Young Men's Organizations also served to consolidate the young business class of a city by introducing them to one another and giving them a common set of cultural activities that, by their very definition, built "character." The reading rooms that flourished with these organizations were regarded as "a pleasant resort and an agreeable place to introduce one's friends and also respectable strangers who visit the city."<sup>35</sup> These organizations provided the setting for Emerson's lectures in the Midwest during the 1850s. Although they shaped the character of popular response to him, he in his turn acted as a catalyst for the cultural consolidation already underway in the region. Emerson's reception in one important midwestern city—Cincinnati—illustrates how these business-oriented audiences helped create an Emerson in line with commercial values.<sup>36</sup>

THE PRIMARY IMPETUS FOR EMERSON'S FIRST TRIP to Cincinnati in 1850 was literary. Emerson's reputation at this time still rested principally on his print production.

*Bulletin*, 23 (1967): 307; and Edwards and Hopewell, *Edward's Great West*, 114, 132–35, 153–54, 185–86, 224, 508, 551–52.

<sup>33</sup> Anne Louise Hastings, "Emerson's Journal at the West, 1850–1853" (Ph.D. dissertation, Indiana University, 1942), 8.

<sup>34</sup> This is the traditional notion of the self-made man, as opposed to the ideal of self-culture. It is discussed at length in Wyllie, *Self-Made Man*; and Cawelti, *Apostles of the Self-Made Man*.

<sup>35</sup> Taylor, *Centennial History*, 241.

<sup>36</sup> Although the responses to Emerson's visits to Cincinnati had at times their own character, they are, so far as I can tell, fairly typical of the range of reaction he received elsewhere in the region. I base this judgment mainly on a wealth of accounts of Emerson's lecture tours in the Middle West and his reception there that rely heavily on local periodicals for documentation: Mead, *Yankee Eloquence*, 24–61; Willard Thorpe, "Emerson on Tour," *Quarterly Journal of Speech*, 16 (1930): 19–34; Samuel P. Orth, *A History of Cleveland, Ohio*, 3 vols. (Chicago and Cleveland, 1910), 1: 491–93; Owen Philip Hawley, *Orient Pearls at Random Strung: Mr. Emerson Comes to Marietta* (Marietta, Ohio, 1967); C. J. Wasung, "Emerson Comes to Detroit," *Michigan History Magazine*, 29 (1945): 59–72; Russel B. Nye, "Emerson in Michigan and the Northwest," *Michigan History Magazine*, 26 (1942): 159–72; Donald F. Tingley, "Ralph Waldo Emerson on the Illinois Lecture Circuit," *Journal of the Illinois State Historical Society*, 64 (1971): 192–205; Paul Russell Anderson, "Quincy, An Outpost of Philosophy," *Journal of the Illinois State Historical Society*, 34 (1941): 54–57; Robert R. Hubach, "Illinois, Host to Nineteenth Century Authors," *Journal of the Illinois State Historical Society*, 38 (1945): 454–59; Hubert H. Hoeltje, "Ralph Waldo Emerson in Iowa," *Iowa Journal of History and Politics*, 25 (1927): 236–76; Hubert H. Hoeltje, "Notes on the History of Lecturing in Iowa, 1855–1885," *Iowa Journal of History and Politics*, 25 (1927): 62–131; Luella M. Wright, "Culture through Lectures," *Iowa Journal of History and Politics*, 38 (1940): 115–62; Brad Luckingham, "The Pioneer Lecturer in the West: A Note on the Appearance of Ralph Waldo Emerson in St. Louis, 1852–1853," *Missouri Historical Review*, 58 (1963): 70–88; "Emerson in Indianapolis," *Indiana History Bulletin*, 30 (1953): 115–16; Lynda Beltz, "Emerson's Lectures in Indianapolis," *Indiana Magazine of History*, 60 (1964): 269–80; Hubert H. Hoeltje, "Emerson in Minnesota," *Minnesota History*, 11 (1930): 145–59; and C. E. Schorer, "Emerson and the Wisconsin Lyceum," *American Literature*, 24 (1953): 462–75. Louise Hastings, "Emerson in Cincinnati," *New England Quarterly*, 11 (1930): 443–69, focuses on Emerson's reception in this city.

In October of 1849, twelve young men—lawyers, clerks, and teachers, none of whom were over twenty-five years of age—formed the Cincinnati Literary Club. These men lived close together and gathered on Friday nights in the rooms of Ainsworth Rand Spofford, a clerk at a Cincinnati bookstore, to eat, drink, and debate slavery, the tariff, and free will. The group combined the aims of self-culture and conviviality that typically characterized Young Men's Associations. It met weekly "to promote the wider culture of our intellectual, moral, and social powers," with one night a month set aside for formal debate and another for the "Informal": songs, light verse, and drinking.<sup>37</sup> As was the case with Young Men's Organizations elsewhere, the young men of the Literary Club could not afford to guarantee Emerson's expenses, so they turned to the "solid men of Cincinnati," lawyers, ministers, and merchants, to underwrite his expenses. The merchants responded by pledging one hundred and fifty dollars toward the course of lectures.<sup>38</sup>

Emerson came to deliver his course of lectures in May of 1850, to a city decidedly unclear as to what to believe about him. He already had a large enough reputation for the *Daily Cincinnati Gazette* to note on 15 May that "the movement for a course of lectures from RALPH WALDO EMERSON, to which we alluded sometime since, has proved successful, and . . . Mr. Emerson will arrive in Cincinnati in a few days, and commence a course of five lectures here early next week." The *Gazette* thought he would have "a few' people to hear him, at least."<sup>39</sup> "In this don't-care-much-for-genius sort of latitude," a Cincinnati correspondent of the *Salem Register* wrote after Emerson's first lecture, "the town was on tip-toe of 'look out' to see what kind of reception would be extended to him, what class of people would attend, and, finally, what would be thought of him. No one could come to any conclusion upon either point from what the daily papers said in advance; for it was observed that they had not been *paid* in advance, and consequently the 'Locals' were as silent as an oyster, excepting so far as they felt called upon to draw attention to his advertisement, &c.—and that, by the way, with the same adjectives that informed us that a *notable* fat boy was exhibiting at the Museum, &c."<sup>40</sup> For the majority of Cincinnatians outside the small literary and professional circles that issued the invitation, the popular attitude was one of wait-and-see. Emerson was but one more presumably famous name, of whom many might have heard but from whom few knew what to expect.

Press reception of Emerson on his first appearance in Cincinnati is significant both because it illustrates the process of public image-making and because it set the tone for Emerson's visits to the region throughout the decade that followed. In the race with other growing cities of the region for resources, midwestern

<sup>37</sup> Cincinnati Literary Club, Minutes, vol. 1, MS., Cincinnati Public Library. On the Cincinnati Literary Club, see Hastings, "Emerson's Journal at the West," 6–7; James Albert Green, *The Literary Club and Cincinnati in 1849* (Cincinnati, Ohio, 1931), 3; and Esie Asbury, "The Literary Club," *Cincinnati Historical Society Bulletin*, 32 (1974): 105–21.

<sup>38</sup> Ainsworth Rand Spofford, "Address Delivered at the Literary Club's 50th Anniversary," *Minutes of the Literary Club*, 28 October 1899, quoted in Hastings, "Emerson in Cincinnati," 443.

<sup>39</sup> *Daily Cincinnati Gazette*, 15 May 1850.

<sup>40</sup> *Salem Register*, 3 June 1850.

newspapers, which were even more intimately connected with the mercantile community than those of the East, became “civic cheerleaders.”<sup>41</sup> Extensive journalistic treatment of the individual lecturers, including Emerson, contributed toward the shaping of a corporate response to the speaker, as had been the case in Britain during Emerson’s 1847–48 tour there.

By far the most common response to Emerson was to wonder why all the fuss about his transcendentalism. “Judging Mr. Emerson’s matter and manner, by this single lecture,” the *Gazette* reporter wrote, “we should write so differently of both, from what we have seen written by others, that the same man could not be recognized as the subject of the several descriptions . . . [H]e is so far, in his intellectual and oratorical lineaments, from resembling the newspaper portraits above which we have at various times seen his name written, that we half incline to think the wrong man has come along, and attempted to play off a hoax upon us backwoods people.” “Gothamite scribes have certainly mistaken Mr. Emerson for somebody else, and given descriptions of him which will not be recognized in this region,” the *Gazette* concluded, referring to descriptions of Emerson that emphasized his religious deviation and his impractical and unintelligible philosophy.<sup>42</sup> Another reviewer, “perfectly satisfied by the Lecture of Wednesday evening,” insisted that “a great deal more nonsense has been written about him by Gilfillan and others, than they have written about other people.” George Gilfillan reviewed Emerson in 1848 for *Tait’s Magazine*, a British periodical, and the *Gazette* reviewer’s familiarity with the British review is perhaps as significant as his disagreement with Gilfillan, who attacked Emerson for triteness, mistiness, and worship of man disguised as nature.<sup>43</sup>

Although Cincinnatians persisted in looking for evidence of Emerson’s vaunted unorthodoxy and fuzzy philosophical doctrines, they could not find it. “In that portion of the discourse which might be placed under the didactic head,” wrote the correspondent for the Cincinnati *Daily Commercial*, after Emerson’s second lecture, “no theory was introduced which would appear to present the lecturer in the character of a ‘new light.’” He was “as unpretending as . . . a good old grandfather over his Bible,” the *Gazette* reported, and “his most remarkable trait is that of plain *common sense*.” The *Columbian and Great West* reported that “the transcendentalism didn’t come, longingly as we looked for it from the beginning, and stoutly as many, who professed to have heard the course before, declared that it would be along by-and-by.” When Emerson’s planned course of lectures met with success, he was persuaded to give a second course of three, “The Natural History of Intellect,” “The Identity of Thought with Nature,” and “Instinct and Inspiration.” In these, it was judged, “our people will get something more of what is *peculiar* in Mr. Emerson’s mind, and philosophical views, than was obtained from

<sup>41</sup> Carl Abbott, *Boosters and Businessmen: Popular Economic Thought and Urban Growth in the Antebellum Middle West* (Westport, Conn., 1981), 39. On the booster mentality on the midwestern frontier, see also Don Harrison Doyle, *The Social Order of a Frontier Community: Jacksonville, Illinois, 1825–70* (Urbana, Ill., 1978), 62–91.

<sup>42</sup> “Emerson’s Lectures,” *Daily Cincinnati Gazette*, 22 May 1850.

<sup>43</sup> *Daily Cincinnati Gazette*, 24 May 1850.



the first course.” Still, newspapers found no sign of a threatening religion or philosophy.<sup>44</sup>

A comparison of the print essays “Aristocracy,” “Eloquence,” “Books,” and “Instinct and Inspiration” with reportage of the lectures that formed their basis offers some idea of what Emerson’s audiences thought they heard if it was not transcendentalism. It is noteworthy that in none of his lectures of the first course did Emerson speak directly about religious or philosophical opinion as he had in lectures of the late 1830s and early 1840s. Rather, he adapted his philosophy to the needs of the popular audience by choosing topics that communicated through concrete and homely metaphors his attitude toward these subjects without ever approaching them directly. His audience believed itself to be getting “common sense, humor, and truth; the second time, humor, truth, and common sense; the third time, truth, common sense, and humor.”<sup>45</sup> The texts of the essays show that the audiences of the lectures were still receiving, albeit indirectly, the characteristic Emersonian depiction of the universe as a series of laws that transcended social convention, tradition, or proscriptive statute. In other words, Emerson’s underlying philosophy and his religious stance in these lectures had not changed substantially from the more controversial *Nature* and “Divinity School Address,” but Emerson was no longer explicitly using the languages of philosophy or religion to make his points.

By applying those laws to subjects that were ostensibly nonpolitical and nonreligious, Emerson seemed to his listeners to be merely passing along practical advice on practical subjects—the epitome of self-culture. In Cincinnati, the talks that received the most enthusiastic responses included “Eloquence” and “England.” “Eloquence” contained what conventionally came to be called “gems” or “pearls of wisdom”: aphoristic sayings that encapsulated the practical laws of human life in a novel way. “England” was praised as “one of the most graphic and interesting pieces of descriptive narration that we have listened to.”<sup>46</sup> It was treated as a catalogue of observations rather than as a coherent piece of thought. If the audience was pleased by Emerson’s “common sense,” it was because his compelling images drawn from everyday life could be understood in a practical, materialist way as well as in the metaphorical, idealist sense in which Emerson probably intended them. Emerson “don’t say at all—he *hints* or *intimates* or walks around about what he *would* say but *don’t* say,” the young Rutherford B. Hayes, a member of the Cincinnati Literary Club, astutely observed in a letter to a friend.<sup>47</sup>

Ironically, one of the least successful of Emerson’s lectures in Cincinnati was concerned with literary culture itself, the area in which Emerson had presumably made his mark. “Books” was pronounced “above the range and without the best of the great majority of the auditory.” His lectures on “The Natural History of the

<sup>44</sup> *Cincinnati Daily Commercial*, 23 May 1850; *Daily Cincinnati Gazette*, 24 May 1850, *Columbian and Great West*, 1 June 1850; *Daily Cincinnati Gazette*, 30 May 1850.

<sup>45</sup> *Columbian and Great West*, 1 June 1850.

<sup>46</sup> *Daily Cincinnati Gazette*, 30 May 1850. For a complete text of “Eloquence,” see *The Complete Works of Ralph Waldo Emerson*, 12 vols. (Boston and New York, 1903–04), 7: 59–100.

<sup>47</sup> *Diary and Letters of Rutherford Birchard Hayes*, Charles Richard Williams, ed., 5 vols. (Columbus, Ohio, 1922), 1: 315.



Intellect," of a more overtly philosophical character, were "of too abstruse a nature, and altogether too comprehensive in their method, to be characterized in a newspaper paragraph or two, at all events from a single hearing." "Instinct and Inspiration," which the printed text shows to be one of the most overtly philosophical and least anecdotal of his lectures, flowed right past the audience. It was what the British and American reviewers would have called "misty"; the Cincinnati audience, at least, the commercial elements of it whose opinions tended to be reflected in newspapers, were by that time prepared for a frontier philosopher rather than a dangerous transcendentalist. Listeners found the lecture difficult and waited for something more to their liking. As Emerson's lectures grew more philosophical and the novelty of having him in town wore off, attendance at his lectures fell.<sup>48</sup>

Throughout the 1850s, midwestern newspapers that reported on Emerson's discourses exhibited some difficulty in summarizing his lectures as coherent wholes. Although audiences were described as "strongly impressed" or "profoundly attentive," reporters often found it "impossible to give a synopsis of the lecture," no matter how favorably impressed.<sup>49</sup> The organizing principles escaped them. Of "The Conduct of Life" in Cincinnati in 1857, for example, the *Daily Enquirer's* front-page story stated: "The lecture was listened to with profound attention, though, from its epigrammatic and somewhat abrupt and disconnected style, it was a matter of extreme difficulty to follow the thread of the discourse."<sup>50</sup> Rutherford Hayes's description of his impression of an Emerson lecture echoes the responses of most newspaper reviewers. "Logic and method, he has none," Hayes wrote, "but his bead-string of suggestions, fancies, ideas, anecdotes, and illustrations, delivered in a subdued, earnest manner, is as effective in chaining the attention of his audience as the most systematic discourse could be."<sup>51</sup> Emerson's philosophy of composition, natural law, and organic growth are clearly articulated elsewhere, and his treatment of individual subjects in the western lectures are without a doubt illustrative of his philosophical framework as he had sketched it in earlier writings. Because he focused on concrete topics for the popular audience, however, the system behind the anecdotes remained implicit and suggestive rather than explicit and logically developed. Audiences frequently reached the conclusion that, in his talks, there was no point at all.

When reporters did summarize Emerson's lectures for their readers, the result was frequently a disjointed series of remotely connected sentences. The following selection, taken from the Cincinnati *Gazette's* summary in 1852 of Emerson's

<sup>48</sup> *Daily Cincinnati Gazette*, 30 May 1850, 3 June 1850. For the printed essay versions of these lectures, see *Works*, 7: 187–222 ("Books"), and 12: 65–89 ("Instinct and Inspiration").

<sup>49</sup> *Chicago Tribune*, 23 January 1857.

<sup>50</sup> *Cincinnati Daily Enquirer*, 28 January 1857.

<sup>51</sup> *Diary and Letters of Hayes*, 301. For a similar reaction, see the commentary of William Cullen Bryant a decade earlier, who, as he listened to Emerson in New York, explicitly listened for transcendentalism: "In regard to the peculiar doctrines of Mr. Emerson, we hardly consider ourselves qualified to judge. We cannot say that we precisely apprehend what they are. Now and then, in listening to his discourses, or reading his essays, we have fancied that we caught glimpses of great and novel truths"; *New York Evening Post*, 3 March 1842, rpt. in Charles I. Glicksberg, "Bryant on Emerson the Lecturer," *New England Quarterly*, 12 (1939): 530–34.

"Wealth," conveys some of the difficulty listeners had in finding an overarching framework in which to put Emerson's anecdotes and aphorisms:

One of the most natural enquiries about a person, but partially known, was "what has been his *success* in life?" The first question asked with regard to a stranger is, "*How does he get his living?*" All men are consumers, and all ought to be producers. Man is an expensive animal and ought to be rich. Wealth has its source in the application of mind to nature. The most intimate ties subsist between thought and nature. The art of getting rich consists, not in industry, but in being at the right spot for such getting, and in the right application of forces. Steam was as abundant 100 years ago as now, but it was not put to so good a use as now. (Applause.) The grass and wheat rots in Michigan, until the active men screw steam power to that hay and flour and whirl it into New York and London. Coals have been rightly called black diamonds. Coal is a portable climate and transports itself. (Applause.) But coal and water were useless in England, till Watt and Stephenson and Brunel came, and then how quickly transformed to wealth!<sup>52</sup>

This summary also affords an interesting comparison of what the audiences heard and what the speaker actually said. The essay "Wealth" from *The Conduct of Life* parallels the lecture in every respect, yet it is fascinating to notice how the newspaper interpretation compares with this printed text:

As soon as a stranger is introduced into any company, one of the first questions which all wish to have answered, is, How does that man get his living? And with reason. He is no whole man until he knows how to earn a blameless livelihood. Society is barbarous until every industrious man can get his living without dishonest customs.

Every man is a consumer, and ought to be a producer. He fails to make his place good in the world unless he not only pays his debt but also adds something to the common wealth. Nor can he do justice to his genius without making some larger demand on the world than a bare subsistence. He is by constitution expensive, and needs to be rich.

Wealth has its source in applications of the mind to nature, from the rudest strokes of spade and axe up to the last secrets of art. Intimate ties subsist between thought and all production; because a better order is equivalent to vast amounts of brute labor. The forces and the resistances are nature's, but the mind acts in bringing things from where they abound to where they are wanted; in wise combining; in directing the practice of the useful arts, and in the creation of finer values by fine art, by eloquence, by song, or the reproduction of memory. Wealth is in applications of mind to nature; and the art of getting rich consists not in industry, much less in saving, but in a better order, in timeliness, in being at the right spot. One man has stronger arms or longer legs; another sees by the course of streams and growth of markets where land will be wanted, makes a clearing to the river, goes to sleep and wakes up rich. Steam is no stronger now than it was a hundred years ago; but it is put to better use. A clever fellow was acquainted with the expansive force of steam; he also saw the wealth of wheat and grass rotting in Michigan. Then he cunningly screws on the steam-pipe to the wheat-crop. Puff now, O Steam! The steam puffs and expands as before, but this time is dragging all Michigan at its back to hungry New York and hungry England.<sup>53</sup>

Some immediately apparent differences between the two texts include a treatment of material facts (steam, wheat, grass, coal) as a more prominent part of the message in the lecture summary and as the substance of the message rather than

<sup>52</sup> *Daily Cincinnati Gazette*, 13 December 1852.

<sup>53</sup> *Works*, 6: 83–124.

as illustrative of higher theory. The audience appears to have been expecting instruction in empirical truth, and that is what they found in Emerson's address. The applause would indicate that the audience responded more readily to the illustrations than to the point of those illustrations: that wealth, both material and moral, consists in the discovery of a "better order" to the one currently in use. Moreover, the newspaper's concern with success is as a how-to proposition rather than as a moral issue having a bearing on one individual's relationship to the social order. For Emerson, the issue is how "every industrious man can get his living without dishonest customs." For the reviewer, it is merely the issue of getting a living. Finally, the newspaper account flattens the double sense of Emerson's utterances. "Every man is a consumer, and ought to be a producer," says Emerson, echoing themes introduced in "The American Scholar," the "Divinity School Address," *Nature*, and "Self-Reliance." In the newspaper summary, the question becomes purely a material one. Emerson's "He is by constitution expensive, and *needs* to be rich," becomes "Man is an expensive animal and *ought* to be rich" (emphasis added). For Emerson, "rich" stands as material metaphor for a spiritual and moral state; in the context in which the reviewer places it, it seems to have predominantly material and economic references.

Emerson meant to inculcate moral reformation through his lecture topics, and he proposed to draw in his audience through a choice of topics that seemed familiar and practical. Some of the titles in his western course, "The Conduct of Life," indicate the nature of the audience to whom he was accommodating himself stylistically—"Power," "Wealth," and "Culture." Each title can be read as praise of the commercial culture as practiced in the United States or, as Emerson intended, a subtle indictment of its shortcomings. Emerson's attempt to restructure his mercantile audience's vision of the institutions they were creating to define their lives might easily be mistaken for endorsement of the existing order. For example, when Emerson says in his lecture on "Power" that "life is a search after power," the audience who heard common sense but no organizing idea may have interpreted the comment as sanction for an aggressive economic expansionism they could readily recognize as a part of their current practice. The organizing idea of the lecture is, however, Emerson's advocacy of a power that derives from a moral understanding of the laws of nature and a "sympathy with the course of things."<sup>54</sup> His larger message has little to do with economics. His analogies, which include much practical advice to young men, are nevertheless taken from the realm of affairs with which his audience was familiar—business.

Emerson may have been systematically misconstrued by his audience. Several instances of newspaper reportage of lectures reveal the same tendencies as are apparent in the Cincinnati *Gazette's* report on "Wealth": a summary of the individual propositions from the lecture without a sense of underlying structure, an inclination to take Emerson's statements at face value as common sense, and a failure to acknowledge Emerson's reasoning by analogy from the material to the

<sup>54</sup> *Works*, 6: 55, 58.

moral sphere.<sup>55</sup> Other newspapers flatly refused to summarize Emerson's discourses and explained their failure to do so in terms that are very suggestive of misunderstanding. The *Alton Weekly Telegraph* (Illinois) is a case in point: "Concerning the matter of Mr. Emerson's lectures, we shall not attempt to speak, as a synopsis of anything so closely condensed would be almost impossible. Each sentence seemed separate and distinct, perfect in itself. On his views, however, of culture, that of polishing our manners so as to suppress all natural and spontaneous emotions—making men mere cultivated automatons—was rather in advance of his audience. They may be capable of being educated to such a point;—but we question its desirability."<sup>56</sup> The irony is, so did Emerson. The discussion either obscures or distorts Emerson's point; from the summary, it is not entirely clear which.

Audience claims that Emerson used no logic in constructing his lectures appear to have been another way of saying that his mercantile audiences could not see the logical structure of the discourses. Stephen Toulmin has analyzed the structure of an argument as consisting of claims (assertions about what is true), grounds (the underlying foundation that assures the claim to be solid and reliable), and warrants (the connection that exists between ground and claim).<sup>57</sup> Emerson's warrant for the assertions of his "common sense" lectures was the radically idealist cosmology sketched out in *Nature* and further elaborated in the spoken and published lectures of the early period. Whatever language or analogies he employed to reach his audience, he saw himself as preaching a message of moral reform whose warrant was a unique spiritual understanding of nature and nature's laws. His audience heard the warrant to be a set of already familiar, pragmatic, common-sense rules for attaining individual financial and social success. The Emerson whose anecdotes and aphorisms are understood but whose larger method is not becomes the epitome of the commercial values prized by the audiences who invited him.

Often presented in the same lecture series with such pragmatic materialists as P. T. Barnum, Emerson's lectures resembled in import, if not in style, Barnum's "Art of Money-Getting," or "Success in Life."<sup>58</sup> Emerson's presence and message became implicated in the expansion of the commercial culture that sponsored his

<sup>55</sup> Instances in which parallel texts can be compared include reportage on "Power" in the *Daily Cincinnati Gazette*, 10 December 1852, with "Power" in *The Conduct of Life* (*Works*, 6: 51–82); and a summary of "Power" in *The Illinois Journal*, 13 January 1853, rpt. in Robert R. Hubach, "Emerson's Lectures in Springfield, Illinois, in January, 1853," *American Notes and Queries*, 6 (1947): 164–67. Compare a summary of "Books" in *The Independent* (New York), 4 April 1850, with "Books" in *Society and Solitude* (*Works*, 7: 187–221). Compare "Social Aims" and "Resources," summarized in *Boston Semi-Weekly Advertiser*, 30 November 1864, with the essays of those titles in *Letters and Social Aims* (*Works*, 8: 77–108, 135–54). Summaries of lectures for which I have not yet been able to locate printed parallel texts include "Education," in *Boston Semi-Weekly Advertiser*, 30 November 1864; "The Man of the World," *Daily Cincinnati Gazette* [same periodical as *Daily Cincinnati Gazette*], 15 March 1867; "Economy," *Daily Cincinnati Gazette*, 14 December 1852; "Culture," *Chicago Daily Tribune*, 4 February 1854.

<sup>56</sup> 20 December 1867, rpt. in Paul O. Williams, "Emerson in Alton, Illinois," *ESQ* [*Emerson Society Quarterly*], 47 (1967): 98. Among those who declined to synopsise because of the "condensed nature" of the utterance were the *Daily Cincinnati Gazette*, 5 June 1850; and the *Cincinnati Daily Enquirer*, 1 February 1857.

<sup>57</sup> Stephen Toulmin, Richard Rieke, and Allan Janik, *An Introduction to Reasoning* (New York, 1979), 25–26.

<sup>58</sup> On Barnum, see Neil Harris, *Humbug: The Art of P. T. Barnum* (Boston, 1973), 154–58, 193–95.

visits. As the lecture system of which he was part became more solidly entwined with the making of the urban commercial order, his lecture performances came to be part of a canon of acquired learning that defined the parameters of knowledge and behavior within the new international bourgeois way of life, and he himself the representative *par excellence* of “culture.”

THE TRANSITION IN THE MIDWEST FROM INSTITUTIONS FOR SELF-CULTURE TO institutions for the spread of culture can be seen in microcosm in the transformation of the lecture into a form of popular entertainment. The process mirrored a larger one, in which “culture” was becoming a form of consumption necessary for the maintenance of one’s class standing. Although originating in the ideals of personal empowerment implicit in the self-culture movement, the new culture industry was signaled by appreciation of emerging icons of culture who apparently supported mercantile values. Emerson’s reputation as American prophet became firmly established in tandem with the rise of a national culture industry that created and perpetuated obedience to social hierarchy.<sup>59</sup> Speakers like Emerson began to be publicized in newspapers and competed with the theater, concerts, panoramas, and wax museums for public audiences. Lecturers were introduced to the public in the same way as other performers.

One criterion that audiences increasingly used to evaluate the worth of performers was the national or international reputation of the performer or amusement in question. P. T. Barnum’s extraordinary engineering of a public reception for Jenny Lind in the United States in 1850 illustrates the influence that journalistic coverage of a public figure could have on a career. Barnum distributed widely a biography of Lind emphasizing her international fame, her piety, character, and philanthropy. Crowds who had not heard of her a few months before mobbed her upon arrival in New York. “All of this extraordinary enthusiasm . . . had developed before Jenny Lind had sung a note in America,” wrote Neil Harris. “In a sense the musical performances, tumultuously received as they were, formed only an anticlimax.”<sup>60</sup>

Lind’s remarkable success as a result of Barnum’s promotion was one version of a national trend toward celebrity-making, which capitalized on gossipy anecdotes designed to reveal the inmost characters of performers. As Emerson entered a company of “distinguished performers and well known names” in Cincinnati, for example, he began to receive a familiar kind of attention in the local press heretofore rare. One “Anecdote of Mr. Emerson” the *Gazette* published during Emerson’s visit to the city depicted Emerson as at once disingenuous, familiar, and eccentric—in short, as a “personality.” His journal was called “a kind of intellectual and scientific rag-bag” in the popular press, his wife an amazed observer of a genius she little understood. The significance of stories such as these lies not so much in any real information they imparted to audiences as in the way

<sup>59</sup> I have borrowed the term “culture industry” from Max Horkheimer and Theodor W. Adorno, *The Dialectic of Enlightenment* (New York, 1969), 120–67.

<sup>60</sup> Harris, *Humbug*, 121.

their personification of Emerson and performers like him met the needs of audiences for personalities (not belief systems) with whom to identify and on whom to model themselves.<sup>61</sup>

Newspapers began to comment frequently on the physical appearance of the speaker as if it were equal in importance to anything he might say. Emerson surprised audiences with his gaunt and homely appearance, his narrow forehead, and his long, hooked nose. In his habitual "plain suit of ill-fitting black," he was "not unlike a New England schoolmaster." He was by turns bashful, ungraceful, embarrassed, and half-apologetic, but each designation only added to his mystique as an uncalculating soul of pure wisdom and character. "He rarely looks his hearers full in the face," the *Gazette* observed, "but at emphatic expression has a habit of turning his eyes backward as if to look in at himself." Here was no trickster or partisan but a single-hearted purveyor of truth.<sup>62</sup>

Response to great individuals became the index of culture in a city, and evidences of "cultured" responses to them became in turn proof of the speaker's own worthiness. The newspapers announced one of a new course of Emerson lectures in 1857 with the assurance that it had been "delivered with great effect before a very cultivated Boston audience." In Chicago, in February of 1854, the *Tribune* prepared the city for Emerson's arrival by reprinting a laudatory excerpt about him from the *Edinburgh Review*, for Chicago, a foreign literary journal of impeccable reputation. Such notices served a dual function: while drumming up interest in a particular speaker, they also reaffirmed for booster-conscious cities their connection with a larger world of "culture" outside the region. Awareness and appreciation of people who had captivated the better sort of audiences elsewhere testified to the tone and quality of the city itself.<sup>63</sup>

The emphasis on a speaker's wide reputation enabled the promoters to clear a profit and established the city's claim to fame as a member of the cultural *avant-garde* but led to the demise of speakers of purely local reputation. As a city's participation in a national system of culture grew, local notables declined in status and authority on the lecture circuit. When the Reverend Francis Vinton of New York came to Cincinnati to lecture on "The Gentlewoman," the *Enquirer* regretted the small crowd in attendance. "The reverend lecturer has not the particular kind of notoriety which, we regret to say, is most attractive here." Cincinnati's own Rev. C. M. Butler lectured on "Sir Philip Sidney" in his home city, and the *Enquirer* took the opportunity to fulminate on the pervasiveness of the celebrity lecture system and its "imported trash."<sup>64</sup> So prevalent did well-known, highly paid speakers become on the western lecture circuit that some newspapers began self-consciously

<sup>61</sup> *Daily Cincinnati Gazette*, 22 November 1852; 15 December 1852. On the tendency toward the creation of media personalities, see Richard Sennett, who, in *The Fall of Public Man* (New York, 1976), made the case that, by the nineteenth century in Western culture, privatization of experience led to a distorted emphasis on personality of public figures rather than attention to their social roles.

<sup>62</sup> *Daily Cincinnati Gazette*, 28 January 1857. Halttunen, *Confidence Men and Painted Women*, 56–152, has commented on the use of dress and mannerisms during this period to communicate an image of personal sincerity.

<sup>63</sup> *Daily Cincinnati Gazette*, 31 January 1857; *Chicago Daily Tribune*, 4 February 1854.

<sup>64</sup> *Cincinnati Daily Enquirer*, 25 January 1857.



to try to stem the tide and restore the old regional system. In 1855, the *Sandusky Register* and the *Genius of the West* both published lists of western lecturers in the hope that the region would begin to employ its own.<sup>65</sup> They were not successful. Westerners continued for the most part to look to those of established reputation, that is, to easterners, to occupy their lecture platforms.

Along with describing visiting speakers in inflated terms, newspapers offered glowing descriptions of the audiences who partook of the high-toned intellectual fare. The more famous the speaker, the more elaborate the description of the audience. "The audience to-night will no doubt be the most brilliant and fashionable that has been drawn together for some time," ran the announcement of a Cincinnati concert of Ole Bull, a Norwegian violinist of renown, and Maurice Strakosch, a pianist, in 1852. Theodore Parker's fame attracted "the select many" to his lecture on "The Progress of Mankind." With Emerson, the praise awarded the audience grew with his national reputation. His first course of lectures in Cincinnati in 1850 attracted an "audience intellectual as well as large." "The literati and the fashion of our Queen city" were expected in December of 1852.<sup>66</sup> By 1857, Emerson's lectures were "largely and brilliantly attended," and it was apparent to the reviewer that "the intellectual aristocracy of the city has seldom been so well represented as in this audience." In Milwaukee, too, Emerson's audience was certified "very large and brilliant." After the Civil War, when Emerson had become a household word as literary figure and popular lecturer, newspaper accounts increasingly congratulated audiences on their wisdom in appreciating such a great man. "The literary public of Cincinnati honored themselves last night, in honoring perhaps the finest scholar and most profound thinker of the country," the *Gazette* reported in the first sentence of its review. "The most elegant assemblages we remember to have seen on any occasion in this city" welcomed him. When Emerson gave his final lecture in Chicago in 1871, it was the audience reaction, not the quality of the lecture, that was at issue. "It is needless to say that it [the lecture] was well received. The applause was discreetly timed, and bespoke the culture of the audience."<sup>67</sup>

The press continually reinforced the renown of the speaker before, during, and after his arrival and thus promoted the notion that the audience was cultured and brilliant. The "justly celebrated Emerson" was praised; journalists were certain that "the fame of the lecturer will undoubtedly draw a crowded hall"; "the poet and philosopher, who is universally recognized as one of the great thinkers of the age"

<sup>65</sup> *Sandusky Commercial Register*, 24 October 1855. Mead, *Yankee Eloquence*, 194–95, discussed the growing disenchantment with eastern lecturers by the end of the 1850s; W. H. Venable, *Beginning of Literary Culture in the Ohio Valley: Historical and Biographical Sketches* (New York, 1949), 251–53, lists the eastern and western lectures being promoted by the various factions.

<sup>66</sup> *Daily Cincinnati Gazette*, 23 and 30 November 1852; 22 May 1850; and 8 December 1852. *Cincinnati Daily Times*, 8 December 1852, printed the only mixed description of an audience I have come across. The auditors were mixed in character, it observed, and "people who never had a Father, and do not possess a thousand dollars in the world to bless themselves and progeny" took the front seats.

<sup>67</sup> *Cincinnati Daily Commercial*, 3 February 1857; *Milwaukee Daily Sentinel*, 8 February 1854; *Daily Cincinnati Gazette*, 15 March 1867; *Cincinnati Daily Commercial*, 15 March 1867; *Chicago Tribune*, 28 November 1871.

was coming to speak.<sup>68</sup> This newspaper promotion could not have created talent where there was none, but it did establish a cycle whereby the repute of the speaker drew self-defined “intellectual” and “cultivated” audiences, and “cultivation” itself began to be defined as attendance on and acquaintance with certain famous cultural figures. Self-culture, the active expansion of one’s faculties and the promoting of self-awareness, was becoming transformed into culture, the conspicuous consumption of the performances of people who were nationally and internationally defined as important intellectuals. Emerson became one of the first symbols of this culture, newly defined as the awareness and mastery of a certain body of knowledge. Culture was a state to be achieved, a status to be acquired, no longer a process of self-awareness and introspection.<sup>69</sup>

Emerson the public personality contributed to a national system of culture that was effectually the consumption of well-known texts and performances. Indeed, his lectures, in an oft-repeated phrase, came to be known as “intellectual treats,” tidbits of wisdom dispensed by the wise man to the public at large. Thomas Wentworth Higginson perhaps best expressed this representative image Emerson had come to hold for most midwesterners as he repeated the assessment of a booking agent for western tours: Emerson’s continued popularity rested “not on the ground that the people understand him, but that ‘they think such men ought to be encouraged.’”<sup>70</sup> “He impresses one with the idea of long years of study, of many nights of toil, of incessant diligence in the fields of art, science and literature,” another commented.<sup>71</sup> Emerson had become the professional embodiment of Man Thinking, the archetype set forth in his own “American Scholar” address of 1837. Ironically, however, in becoming a representative man, he seemed set apart, superhuman—no longer an inspiration to individual thought but an embodiment of the best that had already been thought and was known to be true. As Emerson himself remarked of Horace Greeley, the people liked him because he did their thinking for them.<sup>72</sup>

NEARLY HALF A CENTURY AFTER Emerson’s decade of western lecturing, Thorstein Veblen observed that, in all known civilizations, the people place an esoteric knowledge of truth and reality in the keeping of a select body of specialists: scientists, scholars, savants, clerks, priests, shamans, folk healers. “In the apprehension of the group in whose life and esteem it lives and takes effect,” Veblen wrote, “this esoteric knowledge is taken to embody a systematization of fundamental and eternal truth; although it is evident to any outsider that it will take its

<sup>68</sup> *Daily Cincinnati Gazette*, 10 December 1852; *Chicago Tribune*, 2 February 1854; *Cincinnati Daily Commercial*, 27 January 1857.

<sup>69</sup> This definition of culture as acquisition of a certain body of knowledge and style is suggested by Alan Trachtenberg, *The Incorporation of America: Culture and Society in the Gilded Age* (New York, 1982), 140–81; and Burton J. Bledstein, *The Culture of Professionalism: The Middle Class and the Development of Higher Education in America* (New York, 1976), 1–45.

<sup>70</sup> Thomas Wentworth Higginson, “The American Lecture-System,” *Every Saturday*, 5 (18 April 1868): 494.

<sup>71</sup> *Cincinnati Daily Enquirer*, 1 February 1857.

<sup>72</sup> *Chicago Daily Tribune*, 4 February 1854.

character and its scope from the habits of life of the group, from the institutions with which it is bound in a web of give and take.”<sup>73</sup> Even though Emerson had not necessarily intended to do so, even though, in fact, such a system of culture as an end in itself was at odds with some of the cardinal tenets of his philosophy, he became its high priest. By 1860, it was generally agreed that Emerson was “one of the most remarkable men in America.” He embodied values that his audience took to be vital to their way of life. He was “original, self-reliant, bold in thought and utterance,” yet he seemed to threaten none of the customs or institutions that had become comfortable. He was “unpretending and simple in manners,” yet he became a standard by which to measure one’s own intellectual and moral sophistication. “He says many things that the majority of people either misunderstand or intelligently disapprove,” yet his audience could tell, from his impeccable character and apparent approval of the mores of society—of wealth, power, aristocracy—that he did not really mean these things.<sup>74</sup> He became the image of the seer and prophet whose double-edged moral message could be taken materially if it made more sense to do so. The embodiment of the democratic scholar, he helped to consolidate for an economically defined community a notion of culture that reinforced boundaries between the cultivated and the uncultivated. He represented the paradox of a dominant culture that claimed to be dedicated to self-improvement but that increasingly took self-improvement to mean adherence to an ever-more-clearly defined body of standards and behaviors sanctioned by the mercantile and professional groups who sponsored him.

Emerson could become this powerful national symbol because the structure of the lecture system encouraged speakers and audiences alike to view what happened on the lecture platform as unbiased and apolitical.<sup>75</sup> The moral knowledge dispensed from the platform by the guileless speaker could shape a national consensus. Especially in the Midwest, where the proliferation of religious denominations provided some measure of ideological division, Emerson’s ideas were bled of any philosophical, political, or religious implications and used as the basis for a secular faith that focused on a materially defined progress, unlimited wealth, and conspicuous social achievement within the framework of a stable and proscriptive set of moral values. If professional and mercantile people dominated the values of this new consensus, if the moral orientation of the mercantile community and its Young Men’s Associations was generalized and taken to be impartial and unbiased knowledge, it was not through hypocrisy. The intentions

<sup>73</sup> Thorstein Veblen, *The Higher Learning in America: A Memorandum on the Conduct of Universities by Business Men* (New York, 1957), 1.

<sup>74</sup> *Cincinnati Daily Gazette*, 2 February 1860.

<sup>75</sup> The Cincinnati Young Men’s Mercantile Library Association, for example, took pains to prevent discussion of anything “political” in its lecture program. Orestes Brownson caused a ruckus in 1852 when he made inflammatory remarks about Louis Kossuth. See David Mead, “Brownson and Kossuth at Cincinnati,” *Bulletin of the Historical and Philosophical Society of Ohio*, 7 (1949): 90–93. Henry Ward Beecher was invited to St. Louis in 1860 on the condition that he “eschew all subjects pertaining either to politics or religion”; *Daily Cincinnati Gazette*, 2 February 1860. He refused the invitation. Emerson himself received some difficulty in the press in Cincinnati after he had publicly spoken elsewhere in praise of John Brown. See *Cincinnati Daily Times*, 1 February 1860; and *Cincinnati Daily Commercial*, 2 February 1860.

of the groups were sincere, even if the result was to extend their own hegemony over American culture as a whole.

Certainly, this is not the whole story of Emerson or the whole story of what audiences made of him. There remains the question of what he may have thought of his audience's misapprehension of him. In general, he seems to have believed that a person had to speak the truth and maintain a studied oblivion toward what hearers might or might not make of it.<sup>76</sup> Other audiences contested for the right to interpret his words definitively, among them, religious scholars, philosophers, literary critics, historians, and radical reformers, who did not cease to appropriate his discourse to their own ends, despite the popular triumph of the mercantile Emerson. Because he claimed to speak a truth that transcended context, Emerson may also be a particularly blatant example of the process by which an intellectual may be "made" by the interpretive stance of a specific discourse community. With other intellectuals, the process may not be nearly so clear cut.

A view of the mercantile Emerson nevertheless helps sort out many of the apparent contradictions within Emerson scholarship, as scholars may belong to more than one discourse community at a time. It reminds us that there is one Emerson but many discourse communities to hear his message. It is not only the speaker who represents a battleground in which conflicting cultural tensions or differing discourse communities strive for reconciliation; the text itself is debated and interpreted by various publics. Groups that are catalyzed into new forms of self-consciousness as a result of hearing texts that resonate for them receive ways not only of naming the world for themselves but also of seeming to share a common vision with others who share the text. No matter that the same words may be recognized in a wholly different way by other communities of listeners or readers. If Emerson has come down to us as a cultural prophet, it is not necessarily evidence that we all share a common culture founded on a common intellectual philosophy—only that the words themselves are common to enough cultural groups that we are willing to overlook the sometimes radically different ways in which we hear them.

<sup>76</sup> On Emerson's attitude toward audience reception, see John H. Sloan, "The Miraculous Uplifting: Emerson's Relationship with His Audience," *Quarterly Journal of Speech*, 52 (1966): 10–15. Passages in Emerson's own writing that are illuminating include *The Letters of Ralph Waldo Emerson*, Ralph L. Rusk, ed., 6 vols. (New York, 1939), 5: 4; *The Journals and Miscellaneous Notebooks of Ralph Waldo Emerson*, William H. Gilman, et al., eds., 16 vols. (Cambridge, Mass., 1960–82), 9: 10–11, 50, 225, 258, 430; 10: 315.

## Europeans, Indians, and the Age of Discovery in American History Textbooks

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JAMES AXTELL

IT WOULD BE DIFFICULT TO OVERESTIMATE the role played by textbooks in the teaching of American history. Yet, for some peculiar reason, textbooks are the only products of historical scholarship that do not receive regular critical review by acknowledged experts in the various subfields of American history. Publishers frequently hire pedagogical consultants or teachers from large institutions to critique textbook manuscripts, in order to ensure that the complexity of content and prose style do not overshoot their targeted audiences. But they seldom ask the best scholars to review the books before publication and only occasionally do so for subsequent editions. More to the point, perhaps, the leading historical journals, both general and specialized, do not deign to review textbooks. Only *The History Teacher* regularly extends itself in this direction; even *History: Reviews of New Books* overlooks the survey text in favor of the occasional specialized text and the usual monographs. Regrettably, the new face of the *Journal of American History* will not smile on reviews or even review essays of American history textbooks.

It was this state of affairs—and the forthcoming Columbian hoopla, of course—that prompted the AHA's Columbus Quincentenary Committee to launch this modest, one-person inquiry into what American college students were reading about, not so much Columbus's daring (and ever-controversial) voyages, as important as those were, but the earth-making changes that followed in his wake over the next century and a half. What, if anything, were students learning about the Columbian legacy, the shaping of a new Atlantic (and later global) world that linked the destinies of several continents and myriad peoples? What were they reading about the native peoples of the Americas and Africa who were so suddenly yanked into the European orbit? Were they still being shown the Spanish empire through the distorting lens of the "Black Legend"? Did they ever see the French and other non-English colonizers arrive in the Americas and, if so, with what result? And, perhaps most important, could they, from what they were reading, understand what motivated Columbus, his European successors, and the natives, and what caused the tangle of events to which Americans are collateral if not direct heirs? With these questions in mind, I have read the first chapter or two of sixteen

of the most popular college textbooks currently being used.<sup>1</sup> A few texts were written by one person, but most were collaborative products, whose first sections were authored by specialists in Anglo-American colonial history. Some texts had been around a long time and had seen multiple editions and sometimes revisions; several others were virtually hot off the press.

The similarity of content and treatment is striking. The contents of the sixteen “discovery” chapters are almost interchangeable, especially in the older texts. The standard scenario includes: the native societies of the New World after the Asian migration; European society on the eve of colonization, usually in transition from medieval to early modern; Christopher Columbus and explorers of other nationalities; the Spanish conquest and the establishment of empire; Spain’s colonial competitors—the Dutch, French, and English; and the background of English colonization, including the Reformation, the Armada, and joint-stock companies. Some of these topics receive only a column or two (only three texts treat the reader to full book pages), but most texts touch on them all in some fashion.<sup>2</sup> A few of the newer texts go considerably beyond the standard treatment by adding new topics or by fleshing out some of the traditional topics in greater detail. Gary Nash and Mary Beth Norton have added substantial sections on western Africa in describing America as the commingling of three peoples, while James Henretta has given extensive attention to the social and economic preconditions of European settlement.

All of the texts share two other similarities. The new maritime roads all lead to the new Rome—the founding of the United States via the thirteen English mainland colonies. Although this focus is appropriate for later phases of the subject, it distorts the reality of America’s origins. The Spanish and French empires are introduced only to provide strong contrasts with the emerging Protestant democracy sandwiched between them and never to help explain why the English colonies evolved as they did. The rich island colonies of the West Indies might as well not have existed, for all the reader learns of them. Only the regions of North America north of the Rio Grande and south of the 49th parallel are of interest,

<sup>1</sup> Thomas A. Bailey, *et al.*, *The American Pageant*, 7th edn. (Lexington, Mass.: D.C. Heath, 1983); Bernard Bailyn, *et al.*, *The Great Republic: A History of the American People*, 3d edn. (Lexington, Mass.: D.C. Heath, 1985); T. H. Breen, *et al.*, *America, Past and Present* (Glenview, Ill.: Scott, Foresman, 1984); Richard H. Current, *et al.*, *American History: A Survey*, 6th edn. (New York: Alfred A. Knopf, 1983); John A. Garraty, *The American Nation: A History of the United States*, 5th edn. (New York: Harper and Row, 1983); Rebecca Brooks Gruver, *An American History*, 4th edn. (New York: Alfred A. Knopf, 1985); James A. Henretta, *et al.*, *America’s History* (Chicago: Dorsey Press, 1987); Winthrop D. Jordan, *et al.*, *The United States: Conquering a Continent*, 5th edn. (Englewood Cliffs, N.J.: Prentice-Hall, 1982); Robert Kelley, *The Shaping of the American Past*, 4th edn. (Englewood Cliffs, N.J.: Prentice-Hall, 1986); Edmund S. Morgan, *et al.*, *The National Experience: A History of the United States*, 6th edn. (San Diego, Calif.: Harcourt Brace Jovanovich, 1985); Samuel Eliot Morison, *et al.*, *The Growth of the American Republic*, 6th edn. (New York: Oxford University Press, 1969); Gary B. Nash, *et al.*, *The American People: Creating a Nation and a Society* (New York: Harper and Row, 1986); Mary Beth Norton, *et al.*, *A People and a Nation: A History of the United States* (Boston: Houghton Mifflin, 1986); Stephen Nissenbaum, *et al.*, *The Pursuit of Liberty: A History of the American People* (New York: Alfred A. Knopf, 1984); George Brown Tindall, *America: A Narrative History* (New York: W.W. Norton, 1984); Irwin Unger, *These United States: The Questions of Our Past*, 3d edn. (Englewood Cliffs, N.J.: Prentice-Hall, 1986). For multi-authored texts, I have referred only to the author of the “discovery” chapter(s).

<sup>2</sup> Morison, *Growth of the American Republic*; Nissenbaum, *Pursuit of Liberty*; and Tindall, *America: A Narrative History*, feature full-page texts.



except when “foreign” wars inconveniently spill over the borders. The fluid possibilities of North America in the sixteenth and early seventeenth centuries are simply ignored in the face of the eventual *fait accompli*, and readers can only regard Anglo-America’s imperial rivals as paper tigers.

The textbooks are also stingy with historical time. On the average, they devote only 4 percent of their first-semester pages to the crowded and formative century of discovery, which accounts for some 30 percent of the time between Columbus’s celebrated landing and Reconstruction. The challenge of explaining some of the most complex, important, and interesting events in human history—the discovery of a new continent, the religious upheavals of the sixteenth century, the forging of the Spanish empire, the Columbian biological exchange, the African diaspora—all in twenty or twenty-five pages is one that few, if any, textbook authors have met or are likely to meet. The results thus far are not encouraging.

Even though some authors have treated some subjects better than others have, together, the group of sixteen has committed enough sins of commission and omission to give one serious pause about the possibility of the enterprise as presently conceived. The sins of commission encompass not only an alarming density of factual errors but also ineffective visual material, insensitive characterizations, and insidious half-truths. Virtually no parts of the discovery text are immune to errors of fact. But some subjects seem especially susceptible, particularly those that demand acquaintance with an extensive and active historiography. Among the subjects most vulnerable are Indians, the Spanish empire, and the French colony in Canada (Louisiana and the Illinois country being virtually unheard of until the Anglo-Americans “discover” them late in the eighteenth century). Indians come in for trouble as soon as they decide to leave Asia. Three texts, including that of the renowned sailor, Samuel Eliot Morison, have the first immigrants crossing an unfrozen Bering Strait in boats, in search of political and religious liberty, no doubt, rather than hot meals on the hoof.<sup>3</sup> Another migration occurred, according to two other texts, during or just before the sixteenth century, when the five Iroquois tribes moved from the Southwest—or the Mississippi Valley—into New York State.<sup>4</sup> As it turns out, since at least the 1950s, archaeologists have known that the northern Iroquoian cultures developed *in situ* over many centuries, probably from before the birth of Christ.<sup>5</sup>

Tribal names and linguistic families are additional sticking points. Many texts, especially on their vague, continent-sized maps of native cultures, mangle tribal names, misplace tribal territories, and confuse individual tribes with linguistic groups. Probably because of James Fenimore Cooper, “Mohegans” are forever usurping the land of the “Mahicans” on the upper Hudson; the Hurons, who spoke an Iroquoian language but were the arch-rivals of the New York Iroquois, are seldom placed in their homeland on Georgian Bay in southern Ontario; and the Algonquin tribe on the Ottawa River west of Montreal is too frequently

<sup>3</sup> Bailey, *American Pageant*, 5; Current, *American History: A Survey*, 1; Morison, *Growth of the American Republic*, 3.

<sup>4</sup> Breen, *America, Past and Present*, 6; Jordan, *The United States: Conquering a Continent*, 5.

<sup>5</sup> William C. Sturtevant, gen. ed., *Handbook of North American Indians*, vol. 15: *Northeast*, Bruce G. Trigger, ed. (Washington, D.C., 1978), 322.

mistaken for the language family of Algonquian speakers, who inhabited much of the northeastern woodlands.<sup>6</sup>

Three other common errors creep into discussions of Indians. Several texts assert that the concepts of “private property” and “landownership” were virtually unknown to Indians.<sup>7</sup> On the contrary, although the woodland tribes owned land communally, individuals owned any objects they made and food they procured. House sites, garden plots, fishing spots, and hunting territories were often assigned to families by tribal leaders and were then regarded as “private” as any modern property that is subject to confiscation by a sovereign state. Other texts charge that native government was not truly “organized,” particularly in contrast to the empires of Mexico and Peru, and therefore doomed Indians to “disunity” before the European onslaught.<sup>8</sup> This point of view overlooks the proven effectiveness of noncoercive government in face-to-face societies and of the Creek, Powhatan, Iroquois, and Huron confederacies, which bedeviled various invaders for as long as two centuries. The textbooks characterize native religion as some kind of “primitive” or “pagan” “pantheism” because Indians allegedly believed that everything in nature—the land, trees, rocks, and animals—contained living spirits or souls.<sup>9</sup> Scholars today believe that the natives respected the “souls” or animating “spirits” only of living things, not rocks or land *per se*, and normally supplicated and thanked not individual plants and animals but rather the “boss-spirits” or representatives of species.

THE HALF-TRUTHS ABOUT Indians are perhaps worse than the errors. Despite native population estimates ranging from one to twelve million, several older male authors persist in characterizing the “vast and lonely North American continent” as a “virgin” land, which “like all virgins, inspired conflicting feelings in men’s hearts.”<sup>10</sup> What happened next varies from text to text. Younger authors invariably describe it as rape; at least one of their chivalrous elders preferred to think that “the New World gracefully yielded her virginity” to the conquerors.<sup>11</sup> Another gentleman was obviously relieved that this “vast and virgin continent . . . was so sparsely peopled by Indians that they could be eliminated or shouldered aside. Such a magnificent opportunity for a great democratic experiment,” he chirped with unintended irony, “may never come again.” (This from the same historian who thinks it relevant that Sir Walter Raleigh seduced one of Elizabeth’s maids of honor and Louis XIV had numerous mistresses.<sup>12</sup>

<sup>6</sup> See, for example, Henretta, *America’s History*, 33; Current, *American History: A Survey*, 18; Gruver, *An American History*, 10; and Nissenbaum, *Pursuit of Liberty*, 27.

<sup>7</sup> Bailey, *American Pageant*, 5; Unger, *These United States*, 5; Gruver, *An American History*, 9.

<sup>8</sup> Bailyn, *Great Republic*, 28; Current, *American History: A Survey*, 17; Morison, *Growth of the American Republic*, 12; Tindall, *America: A Narrative History*, 10.

<sup>9</sup> Henretta, *America’s History*, 44; Jordan, *The United States: Conquering a Continent*, 5; Unger, *These United States*, 5.

<sup>10</sup> Morgan, *The National Experience*, 12; Garraty, *The American Nation*, 8.

<sup>11</sup> Morison, *Growth of the American Republic*, 19.

<sup>12</sup> Bailey, *American Pageant*, 2, 9, 42.

Equally misleading is Bernard Bailyn's statistical apology for English colonization, much in the manner of John Winthrop's argument from *vacuum domicilium*. In 1600, Bailyn wrote, the 362,000 square miles of the eastern seaboard were "largely uncultivated" by the Indians (as indeed they still are today). Somehow, he knew that "nowhere was more than 1 per cent of all the land available for cultivation being farmed." East of the Appalachians, the average population density, he tells us without interpretive comment, was thirty-four persons per hundred square miles. In New England, the average density was between four and five persons per square mile, as if the student knew whether that was sparsely or densely settled for a semi-hunting-and-gathering society. Consequently, although Bailyn acknowledged that the natives sensibly concentrated their semi-permanent settlements in "the fertile coastal plain and the broad river valleys," he concluded, as did the Puritans, that "the Indians' hold upon the land was light."<sup>13</sup>

Other misleading assertions abound. The (illiterate) chief Powhatan "signed" a peace treaty with the Virginians in 1614 and "sealed the deal in traditional fashion by marrying his daughter Pocahontas to John Rolfe."<sup>14</sup> Not only was no treaty ever signed but Powhatan ceased his efforts to evict the English trespassers only after Sir Thomas Dale's troops had pummeled his villages relentlessly for two years, and Pocahontas had been kidnapped by the English as a hostage for peace, converted (when her father refused to come up with the ransom), and married (which Powhatan did not witness, for fear of his life).<sup>15</sup>

FOR SOME OF THE SAME REASONS, THE FRENCH AND SPANISH do not fare much better at the hands of Anglo-American historians. Sad to say, the "Black Legend" and others still warp the treatments of the Spanish empire. Typical is Bailyn's, which, despite its careful attention to Spanish administrative successes over a vast area, leaves no doubt that the Spanish story is primarily a "tale of slaughter and conquest" with "no heroes." "Half-mad with greed," *conquistadores* were "unbelievably determined," "courageous," "fierce," "brutal," and "ruthless," characterizations echoed in most of the other texts without Bailyn's compensatory treatment of Spanish achievements after the conquest.<sup>16</sup> Under the spell of the legend, truth gives way to fiction. One text asserts that Spain claimed all of the New World "except for a chunk of it (Brazil) that they"—not Pope Alexander VI—"left to the Portuguese."<sup>17</sup> Another text vows that Cortés burned—not scuttled—his ships in Veracruz harbor. The same text says, without indicating the period, that some 750,000 Spaniards emigrated to the New World, another that "few" were "able or willing to emigrate."<sup>18</sup> The latest estimates suggest less than 250,000 in the

<sup>13</sup> Bailyn, *Great Republic*, 28.

<sup>14</sup> Norton, *A People and a Nation*, 23.

<sup>15</sup> Philip L. Barbour, *Pocahontas and Her World* (Boston, 1970), chap. 10.

<sup>16</sup> Bailyn, *Great Republic*, 6–14.

<sup>17</sup> Current, *American History: A Survey*, 8.

<sup>18</sup> Breen, *America, Past and Present*, 16, 18; Current, *American History: A Survey*, 9.

sixteenth century, another 200,000 in the first half of the seventeenth.<sup>19</sup> Perhaps the most common misunderstanding about Hispanic America is the equation of the *encomienda* system with simple land grants.<sup>20</sup> On the contrary, land was plentiful but worthless without native labor. The Spanish colonists therefore requested grants of specified Indian villages, which would supply them, landholders who lived elsewhere, with tributary goods and labor to work their farms and industries.

If possible, the French suffer more misunderstandings and errors than do either Indians or Spaniards. Considering the relatively small cast of characters on the Canadian stage, one would think that the skimpy program notes offered by these authors would be more accurate. More than one text has Giovanni da Verrazzano (usually spelled incorrectly, with only one 'z') sailing in 1523 instead of 1524, and Jacques Cartier exploring the St. Lawrence "as far inland as present-day Montreal" on his first voyage in 1534 rather than a year later on his second voyage.<sup>21</sup> Cartier did not winter "near the mouth of the Saguenay," as Morison inexplicably asserted, but much farther upstream on the St. Charles River at today's Quebec City.<sup>22</sup> His famous complaint about "the land God gave to Cain" referred to Labrador, not Newfoundland.<sup>23</sup> And the statement that Cartier's voyages "did not lead immediately to colonizing efforts" is incorrect.<sup>24</sup> In 1541–42, a third expedition, totaling eight ships under Cartier and le sieur de Roberval took several hundred colonists of both sexes to the St. Lawrence, along with livestock and farm equipment. When the colonists scampered home after one winter, the French did not, as Edmund Morgan suggested, "forget" North America "for the rest of the century."<sup>25</sup> In 1562, they established an ill-fated colony of Huguenots in Florida, and, in the 1580s, merchant ships returned to the St. Lawrence in number to reestablish a lucrative fur trade.

The next textbook star of Canadian history, Samuel de Champlain, is equally tarnished by myth and error. Champlain was an intrepid traveler, but his explorations could never have extended French claims "as far inland as Wisconsin," as Thomas Bailey alleged they did, because Champlain never got farther west than Georgian Bay on Lake Huron.<sup>26</sup> Nor was he personally responsible for earning New France "the lasting enmity of the Iroquois tribes" in 1609 by his actions at the battle of Lake Champlain.<sup>27</sup> If he wished to procure the rich fur trade of the north for his employers, he had no choice but to ally the French with the Montagnais and Algonquin hunters on the north shore of the St. Lawrence and

<sup>19</sup> *Cambridge History of Latin America, II: Colonial Latin America*, Leslie Bethell, ed., (Cambridge, 1984), 16.

<sup>20</sup> Bailyn, *Great Republic*, 12; Gruver, *An American History*, 19; Morison, *Growth of the American Republic*, 22.

<sup>21</sup> Unger, *These United States*, 17; Nissenbaum, *Pursuit of Liberty*, 35; Garraty, *The American Nation*, 10; Morgan, *The National Experience*, 13.

<sup>22</sup> Morison, *Growth of the American Republic*, 32.

<sup>23</sup> Breen, *America, Past and Present*, 19.

<sup>24</sup> Unger, *These United States*, 17.

<sup>25</sup> Morgan, *The National Experience*, 13.

<sup>26</sup> Bailey, *American Pageant*, 42 (picture caption).

<sup>27</sup> Current, *American History: A Survey*, 19; Bailey, *American Pageant*, 43.

with their traditional friends, the Hurons (who were not, as Bailey suggested, “nearby” but several hundred river miles away).<sup>28</sup> Long before Champlain and his men fired on the Iroquois at Lake Champlain, the French had cast their lot with ancient enemies of the Iroquois.

Most textbooks do not extend themselves beyond the founding of Quebec, but those that do continue to pattern the French story after the dusty scenarios of Francis Parkman and George Wrong. This is puzzling because most chapter bibliographies—which obviously change much faster than the texts themselves—contain the corrective works of William Eccles.<sup>29</sup> It is a mystery how anyone, after reading Eccles, can write that the French *habitants* were “serfs” or “peasants,” that New France was a “feudal” society under an “almost completely autocratic regime,” that the French colonies “never prospered,” that the French population was only 2,000 when the crown took over in 1660 (it was 1663), that French *coureurs de bois* brought packs of “deerskins” to Montreal on “packhorse,” that the Jesuit missionaries “made few permanent converts,” and that “the natives were folded into the French Canadians,” in Morison’s half-baked simile, “as a pastry-cook folds butter into pie-crust dough.”<sup>30</sup> Anyone who can write, as Bailey did, that “the lasting effect” of French influence, “except in Canada [!], was not great” has obviously never visited Louisiana, the Great Lakes states, northern New England, or the old Illinois country around Ste. Genevieve and Kaskaskia.<sup>31</sup>

MORE THAN ERRORS AND MISCONCEPTIONS MAR THE AVERAGE textbook account of the Age of Discovery. Most alarming to the ethnohistorian are ethnocentric or inaccurate characterizations of Indians. Authors who still use words and phrases like “red man,” “superstitious,” “primitive,” “half-breeds,” “massacre,” “French and Indian wars,” “war-whooping,” “feathered foes,” “painted allies,” and “tawny-skinned pagan aborigines” need a crash course in cultural relativism and ethnic sensitivity.<sup>32</sup>

Of greater concern, perhaps, is the ineffective, misleading, often distracting use of visual materials. It is difficult not to share Jacques Barzun’s recent regret that the heavy use of visuals encourages the unfortunate habit of hop, skip, and jump in “the most continuous [and] integrated of all subjects.” “Each double spread in quarto size,” he has written, “is filled with pictures, maps, charts, and diagrams in four colors. Among these islands of attraction there is a black and white river of printed text meandering irregularly and looking as superfluous as the prose of a

<sup>28</sup> Bailey, *American Pageant*, 43.

<sup>29</sup> William Eccles, *The Canadian Frontier, 1534–1760*, rev. edn. (Albuquerque, N.M., 1974); *France in America* (New York, 1972), and others.

<sup>30</sup> Bailey, *American Pageant*, 43–47; Breen, *America, Past and Present*, 19; Gruver, *An American History*, 23; Henretta, *America’s History*, 36–37; and Morison, *Growth of the American Republic*, 13.

<sup>31</sup> Bailey, *American Pageant*, 45.

<sup>32</sup> Bailey, *American Pageant*, is the worst offender (43–45, 48, 51), but see Henretta, *America’s History*, 19, 32, and Morgan, *The National Experience*, 14 (“roamed”). On the importance of language, see James Axtell, “Forked Tongues: Moral Judgments in Indian History,” *Perspectives: AHA Newsletter*, 25 (February 1987): 10, 12–13.

good display ad. Picture and caption do all the work.”<sup>33</sup> There is no doubt that publishers go too far in this glitzy direction. Whole-page pictures, particularly of simple portraits, are unnecessary, especially when detailed maps and engravings often cry out for enlargement. When the portrait features an anonymous child pointing to a contemporary Latin map of the Chesapeake, both obviously without historical importance or relevance to the text, you know that Madison Avenue has invaded the offices of the publisher.<sup>34</sup>

Worse yet is the use of fictionalized, nineteenth-century engraved portraits from the Bettmann Archive and pictures that do not teach. Indian baskets and bowls, no matter how beautifully wrought, are boring and pedagogically useless, as are John White’s drawing of an Indian couple eating, an unfocused, ultra-linear rendition of Tenochtitlán from *National Geographic*, a Massachusetts Bay Colony seal on which the Indians’ alleged request “Come over and help us” is illegible, and an engraving of the ordinary exterior of the great silver mountain at Potosí.<sup>35</sup> Poor captions only compound the injury inflicted by ill-chosen illustrations. The use of Théodore de Bry’s engraving of a Florida Indian farming scene to illustrate the sexual division of native labor is misleading if the caption does not also point out that the artist depicted the fields as plowed European-style rather than hilled Indian-style and the horseshoe-crab hoes as one-piece monstrosities.<sup>36</sup>

As someone who uses hundreds of slides in his teaching, I applaud the judicious use of visual materials in history books of all kinds. Despite some exceptions, the sixteen textbooks under study contain some excellent illustrations. Among the most evocative and useful are new maps of western African cultures and slaving ports, the world known to Europeans in 1492, and a satellite view of North America in 1650 looking west from Europe; Indian smallpox victims from an Aztec codex and a syphilitic man by Dürer; engravings of Luther in the pulpit overlooking Catholic officials in the mouth of hell and of an eighteenth-century western African village beside a field of Indian corn; a fancy German box of Oriental spices displayed like jewels; and a colorful painting of Canadian *voyageurs* pulling canoes on sledges over a frozen river.<sup>37</sup> These illustrations are keyed to the text and served by ample captions that highlight their historical (rather than aesthetic) value. Although they do interrupt the flow of the printed page, as Barzun lamented, they are capable of teaching video-bred students about novel subjects in ways that prose alone can seldom match. When they are chosen and captioned well, they have the power to reify and reinforce the imaginative word.

<sup>33</sup> Jacques Barzun, “Teaching and Research in History Today,” *History Teacher*, 19 (1986): 523.

<sup>34</sup> Bailyn, *Great Republic*, 7, 29. The Bailyn text has at least two poorly cropped pictures that run into the binding (19, 67).

<sup>35</sup> Nash, *American People*, 5 (baskets), 19 (Tenochtitlán), 21 (Potosí); Norton, *A People and a Nation*, 30 (bowl); Bailey, *American Pageant*, 8 (couple); Henretta, *America’s History*, 43 (seal).

<sup>36</sup> Norton, *A People and a Nation*, 6; Breen, *America, Past and Present*, 5.

<sup>37</sup> Nash, *American People*, 9 (ports), 20 (smallpox); Norton, *A People and a Nation*, 12 (village), 18 (smallpox); Jordan, *The United States: Conquering a Continent*, 7; Bailey, *American Pageant*, 3 (1492); Henretta, *America’s History*, 37 (1650); Unger, *These United States*, 7 (spices), 20 (Dürer); Tindall, *America: A Narrative History*, 31 (Luther); and Breen, *America, Past and Present*, 20 (canoes).



IN READING THE SIXTEEN TEXTS, I was most struck not by misinformation and misconceptions, as plentiful as those were, but by lost opportunities. The authors' sins of omission seemed then, and seem now, much more serious than the venial sins of commission to which historians are all more or less prey. The things I missed in most of these textbooks are any hints of lively prose, evocative details, and an occasional quotation from contemporaries to relieve the tedium of arid generalizations and omniscient paraphrase. Unfortunately, when five or six of the most momentous events in Western history are crammed into twenty-five pages, nearly half of which are consumed by illustrations, liveliness and detail are the first victims.

As for missing contents, the list is rather long. Acceptable treatments are beginning to appear of the Irish precedents for English colonization, the African diaspora, disease, and various facets of mutual acculturation.<sup>38</sup> Two textbooks, those of Stephen Nissenbaum and Samuel Eliot Morison, even do justice to Columbus himself.<sup>39</sup> Yet the Age of Discovery is not fully explicable without attention to at least nine other subjects.

First, we textbook writers and teachers have to explain *why* Europeans needed spices, silks, furs, and fish; young, twentieth-century Americans simply do not know the sixteenth-century uses of such things. Second, Columbus's voyages to America are more faithfully rendered when put in the context of the crusading mentality of the Spanish *Reconquista* and of the Iberian experience in colonizing the islands of the eastern Atlantic. The Spanish expulsion of the Moors and Jews and the conquest of the Canaries led directly to the reduction of the West Indies.

Third, since so much of the sixteenth century was devoted to maritime enterprise, we need to evoke for our landlubbing students the smells, noises, and colors of the sea, the volatile forces of wind and water, the constraints and opportunities of nautical technology and navigation, and the living conditions and jobs aboard various kinds of sailing ships. We also need to make them regard the Atlantic as a highway rather than a moat. Fourth, we need to evoke the changing geographies of our historical settings. As Captain John Smith warned in 1624, "History without Geography wandreth as a Vagrant without a certaine habitation."<sup>40</sup> Richard Current was the only textbook author who paid the least attention to the geography of colonial North America and then only in a separate, generalized section.<sup>41</sup>

Fifth, although texts are offering somewhat more accurate generalizations about Indians, we also need to portray more accurately their politics, religions, economies, and warfare (which usually appears only as a noisy, hair-raising melee). To understand the making of Anglo-America is impossible without close and

<sup>38</sup> Breen, *America, Past and Present*, 7, 10 (disease), 8–9 (acculturation), 25–26 (Ireland); Kelley, *Shaping of the American Past*, 9, 12 (Ireland); Nash, *American People*, 9–11, 63–74 (Africa), 20 (disease); Norton, *A People and a Nation*, 9–13, 47–53 (Africa); Tindall, *America: A Narrative History*, 19–22 (acculturation and disease).

<sup>39</sup> Nissenbaum, *Pursuit of Liberty*, 1–15; Morison, *Growth of the American Republic*, 15–22.

<sup>40</sup> *The Complete Works of Captain John Smith (1580–1631)*, Philip L. Barbour, ed., 3 vols. (Chapel Hill, N.C., 1986), 2: 338.

<sup>41</sup> Current, *American History: A Survey*, 15–16.

sustained attention to its indigenous predecessors, allies, and nemeses.<sup>42</sup> A sixth need is to describe at some length post-conquest society in the Spanish empire and the nature of nonmilitary Indian relations, lest the Black Legend continue to skew our perspective and feed our WASPish delusions. Perhaps we could begin to remedy the myth of Nordic superiority, as Salvador de Madariaga once suggested, by hanging our maps with the South up and the North down.<sup>43</sup>

Seventh, it is time to integrate the Caribbean colonies into the history of North America, rather than relegating them to some warm-watery limbo. They were never far from the thoughts of colonial administrators and investors, and they should be as close to ours. In treating the West Indies, we should certainly describe the economic and political importance of privateering, which helped underwrite several mainland colonies. Eighth, since American history textbooks dwell on the evolution of the United States, we would do well to pay much closer attention to the Spanish borderlands in the Southeast and Southwest. We need more than the obligatory bow to St. Augustine and Coronado if we are to explain the nation's evolving history and the enduring Hispanic legacy in those regions. How refreshing it would be to find a textbook that began on the West Coast before treating the traditional eastern colonies and then worked in opposite directions toward a late eighteenth-century meeting at the Mississippi.

To make it work, we need to fill the ninth and, to my mind, largest gap—the full story of French experience, not only in Canada but also in the Great Lakes and the Ohio, Mississippi, and Missouri valleys. Textbooks must explain why, suddenly in 1689, the French set upon the hapless English colonies, as well as what the French had been doing in Canada since Quebec was founded and how they procured their Indian allies. In a battle for pages, I would argue that the French affected the course of North American history much more than did the Spanish and therefore deserve considerably more coverage than they are currently getting, which is an error-ridden pittance. If we could address all of these important omissions, our students might have a fighting chance to grasp the deep significance of the sixteenth century, not only for American history but for the history of the world. As it is now, they are bound by the vital omissions from and gagged by the stylistic aridity of our textbooks.

WHY ARE THE OPENING CHAPTERS OF OUR TEXTBOOKS SO INADEQUATE? The task of explaining the Age of Discovery is made largely impossible by the way we divide our survey courses at Reconstruction. This arbitrary division forces us to cover nearly 400 years in the first semester and little more than a fourth as much time in the second. Not only is this pedagogical nonsense but it smacks of egregious whiggism as well: the closer we get to the present, the more important events

<sup>42</sup> James Axtell, "Colonial America without the Indians: Counterfactual Reflections," *Journal of American History*, 73 (1987): 981–96.

<sup>43</sup> Philip Wayne Powell, *Tree of Hate: Propaganda and Prejudices Affecting United States Relations with the Hispanic World* (New York, 1971), 6.

become. It also ignores the origins of American society, which, like all origins, contain the seeds, roots, and even stems of later developments.<sup>44</sup>

Another unfortunate constraint on the first chapters is the textbooks' national focus. If we enter the New World in 1492 blinkered by the future boundaries of the United States, we are unlikely to do justice to the complex relations between geography, history, and culture on that vast hemispheric stage or to the non-English colonizers and natives who played their own important parts on it. To these reasons, we should add the peculiar pressures on academic publishers, who are often forced by politics and mass-market economics to pitch their products to the lowest common denominator. Thus, language, explanation, organization, and controversy are oversimplified. As the "lightning rods of American society," textbooks frequently attract too much social static to allow them to present the full and complicated truth about the past.<sup>45</sup>

Closer to home, many of our failures in writing textbooks are attributable to the authors' relative lack of preparation. The writers of single-authored texts are seldom trained in colonial history, and the colonial experts of multi-authored texts are seldom familiar with the sixteenth century. Since the Boltonian generation, American colonial historians have virtually neglected the histories of non-English empires—the Spanish, Dutch, and Portuguese but particularly the French. Because more colonialists were trained in New England and the Northeast, they absorbed the residual Black and Black Robe legends of their mentors, who were nursed on William H. Prescott and Francis Parkman. The result is a regrettable if understandable ignorance of the historiographies of Hispanic and Franco-America, not to mention maritime history and ethnohistory. If the bibliographies of the textbook chapters are any indication, their authors are not reading the latest or best literature, or, if they are, their notes are not making it to the text. Of course, some acquaintance with the primary sources of the period would serve as a firm rudder in the shoals of a vast and turbulent historiography. With the publication of David Quinn's five-volume *New American World* and John Parry's and Robert Keith's *New Iberian World*, authors no longer have an excuse to procrastinate.<sup>46</sup>

It behooves me to end on a more positive note and to offer some concrete suggestions to the authors, publishers, and users of textbooks. As a bare minimum, we need to double the space allocated to the Age of Discovery. Even if no one else had written a word, Quinn's twenty-four books should force American historians to take seriously the sixteenth century and its colonial legacies. It follows that multi-author textbook teams should include a comparative colonialist to handle the first 200 years, roughly from the 1480s to 1689. It is simply unreasonable to expect one person to master the first 300 years of American history, including the American Revolution, when two, often three, people are needed to cover the last

<sup>44</sup> Carl Bridenbaugh, "The Neglected First Half of American History," *AHR*, 53 (1947–48): 506–17; James Axtell, "A North American Perspective for Colonial History," *History Teacher*, 12 (1979): 549–62.

<sup>45</sup> Frances FitzGerald, *America Revised: History Schoolbooks in the Twentieth Century* (New York, 1980), 42.

<sup>46</sup> David B. Quinn, ed., *New American World: A Documentary History of North America to 1612*, 5 vols. (New York, 1979); John H. Parry and Robert G. Keith, eds., *New Iberian World: A Documentary History of the Discovery and Settlement of Latin America to the Early 17th Century*, 5 vols. (New York, 1984).

125 years. And, although a broad knowledge of the colonial Americas is required, no one should be hired to write a textbook who does not possess enough imagination and stylistic punch to capture and hold the fractured attention of TV-teethed and Walkman-weaned teenagers.

My advice to these scholarly paragons is to buck tradition and insist on two freedoms: first, total responsibility for choosing illustrations, drafting maps, and writing captions, so as to integrate all three with the text; second, the right to restore to the text page a modicum of historical concreteness and fuller explanations of causation, even at the risk of sacrificing some topical coverage or visual material. To their publishers, I offer two related suggestions: give authors more conceptual and stylistic rein, and, when the manuscript is submitted, ask experts on exploration, Indians, European empires, and African history to critique it with as much rigor as an honorarium can buy.

Finally, for the adopters and readers of textbooks, I have two suggestions. Stop adopting textbooks that are hopelessly outdated, stylistically painful, and cratered with crucial omissions. By exercising your options in the free market, you can eventually secure the pulping of the worst books and initiate the writing of better ones. Second, pressure the better journals into the regular reviewing of textbooks. Specialized journals such as the *William and Mary Quarterly*, *Journal of the Early Republic*, *Ethnohistory*, and *Labor History* should be encouraged to commission periodic review essays of the treatment of their specialties by the current crop of textbooks, similar to this initiative. Such a strategy would do much more for the future quality of our textbooks than would the generalized overviews offered by *The History Teacher*. If our goal is to capture new audiences for history, we must first ensure that we do not alienate the one captive audience we have.

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## Reviews of Books

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### GENERAL

PETER GAY. *Freud for Historians*. New York: Oxford University Press. 1985. Pp. xx, 252. \$17.95.

The thesis of Peter Gay's delightful little book is that historians and psychoanalysts have more in common than they realize. They share the same subject matter: both are concerned with human beings and the influences that shape their lives. Both have a remarkably similar approach to their subject, for historians, like analysts, adhere to some sort of theory (whether explicitly stated or not) about human nature. Both attribute motives to their subjects, deal with emotions and passions, attempt to analyze irrationality, and assume that all people—whatever their time frame or cultural setting—display certain stable and discernible traits. Historians and psychoanalysts know that causation is the most interesting and difficult problem they confront: why did that development take place, why did that person act that way? Finally, good historians, like good analysts, deplore reductionism and oversimplification, for both agree with Sigmund Freud that we are complex creatures of concealment and contradiction, whose motives and actions are *überbestimmt*, having multiple causes and manifold meanings.

If the two disciplines have so much in common, why do historians resist being enlightened by psychoanalysts? In part, no doubt, it is because so much shoddy history has been written by so-called psychohistorians. It is often, as Russell Baker once said, Freudulant. But claptrap can be found in every discipline. To be aware of the "fiascos of psychohistory," Gay notes, is "not to yield the ground but to clear it." (p. 184).

Gay stresses a deeper reason for resistance. He thinks that historians generally know little about Freud or have misread or misunderstood him. He gives short shrift to the willfully ignorant who (like J. H. Hexter and Geoffrey Barraclough) "parade their ignorance as a badge of professional wisdom" (p. 9) or those (like Kenneth Lynn and David Stannard) who, having made "nonsense of Freud have had no

trouble demonstrating that Freud is talking nonsense" (p. 31).

Gay deals constructively and candidly with genuine difficulties historians have found with Freud, and his tone is neither argumentative nor flippant. To those who have concluded that Freud's theories were applicable only to a limited group of people in a restricted historical environment (hysterical upper-middle-class women of *fin-de-siècle* Vienna) Gay points out that Freud's patients included Russians and English, princesses and housewives, a five-year-old boy, and an American poet. After World War I he conducted more analyses in English than in German.

Gay shows that Freud was responsive to new evidence and not nearly as inflexible as is generally imagined. Indeed, Gay argues that "the history of psychoanalysis is, for the first four decades, largely the history of Freud changing his views" about analytic theory, the structure of instincts, anxiety, and female sexuality (p. 60). And yet Freud and his successors have indeed been dogmatic about one essential claim: "all humans have some inescapable, universal pre-conditions" (p. 88). But that is emphatically not to say that Freud failed to recognize the diverse ways people responded to instinctual drives. He saw drives as at once uniform and varied. Thus, both psychoanalysts and historians recognize the force of change and development as well as continuity. Human experience, Gay concludes in an apt metaphor, is like a game of chess: from a few elements and a handful of prescribed rules, we construct lives of inexhaustible variation.

Some historians, when baffled by pathological characters or irrational behavior, admit that Freudian analysis may be used as a last resort, but they insist that they do not need psychiatric help in dealing with the behavior of normal people. Gay admonishes us that what we may call "normal behavior" or "rational self-interest" is more complex and less rational than meets the reductionist's eye. The lust for gold, for instance, is no single-minded or hard-headed drive for material wealth. As affluent and thoroughly miserable workaholics mani-

fest, greed is a response to a wide variety of human needs including that of placating anxiety. Historians have not adequately understood the psychodynamics of "naked self-interest," and neither, Gay notes, have psychoanalysts.

Gay is less successful in responding to the objection that psychoanalysis, admittedly useful in understanding the behavior of individuals, has little to offer but vacuous generalities when applied to groups. Gay offers graceful and thoroughly appropriate comments about the dangers of generalizing from the particular and about the interaction between the self and the social environment, but he never examines the tough problems Freud posed when he declared that "group psychology is at the same time individual psychology." The author's discussion of the psychodynamics of groups is particularly disappointing because he has said that the major purpose of this book is to provide the theoretical justification for his ambitious study of nineteenth-century bourgeois culture (p. xii).

Happily, he does not leave us wandering about uncertainly in a theoretical haze. Instead, he takes the advice of William James, whose trenchant comment appears under the dedications of this book: "acts and examples stay." Gay provides us with luminous examples of how historians well versed in psychoanalysis can move beyond biography to illumine broader social canvasses. His illustrations include a discussion of E. R. Dodds's superb study, *The Greeks and the Irrational* (1951), and John Demos's masterly work, *Entertaining Satan: Witchcraft and the Culture of Early New England* (1982). The advantages brought to historical writing by a skillful and prudent use of psychoanalytical insights can be seen clearly in comparing two noted historians' discussion of the causes of the persecution of witches in the seventeenth century. Hugh Trevor-Roper's suave and facile treatment of this illusively complex phenomenon is simply not as good as Demos's thorough analysis, which draws on his command of three disciplines: history, anthropology, and psychoanalysis. Dodds, Demos, and Gay himself have demonstrated that historians can use psychology effectively without abandoning previous training. They use psychology as a supplement to, not as a substitute for traditional approaches.

I have one complaint. Gay's commitment to Freud prevents him from doing justice to developments in psychoanalysis since the death of the great man. He seems totally unaware of the advances that have been made in personality theory and therapy over the past half century. For example, Freud's conception of the centrality of instinctual drives is now seriously questioned. Gay's assertion that there is only one "psychoanalytic view" of instinctual drives is excessively—and certainly uncharacteristically—simplistic (p. 91). Further, not all psychoana-

lysts maintain that drives occupy "center stage" (p. 89). Demos's book, for example, relies on a number of post-Freudian psychoanalytic theories, most notably that of Heinz Kohut, who rejected Freud's concept of psychobiological drives. The irony is striking: Gay wants to give historians a new and broader perspective; yet, by confining himself to orthodox Freudian analysis, he has seriously restricted the breadth of view he is endorsing.

Despite its surprising omissions, we should all be grateful for this graceful, wise, and witty essay that makes us more sensitive to the complexities of the human experience and urges historians to join forces with psychoanalysts in an amicable search for the truth about the past.

ROBERT G. L. WAITE  
Williams College

GERHARD KLUCHERT. *Geschichtsschreibung und Revolution: Die historischen Schriften von Karl Marx und Friedrich Engels 1846 bis 1852*. (Problemata, number 103.) Stuttgart: Frommann-Holzboog. 1985. Pp. 409.

The purpose and content of this book are—for once—accurately conveyed by its title. Gerhard Kluchert aims to clarify the role of the writing of history in the work of Marx and Engels and thereby contribute to an understanding of the development of their thought in general. He concentrates on their writings immediately before and after the revolutions of 1848 and explicates (in 400 pages of close type with a plethora of detailed footnotes) the changing interaction of historical interpretation and political perspective during these years. Although Kluchert's researches are not quite as novel as he claims (particularly given the vast numbers of other laborers in this field cited in his footnotes), this is the most thorough discussion of the subject to date. Although they cover much the same ground, the works of Alan Gilbert and Richard Hunt concentrate more on the political (rather than historical and economic) perspectives of Marx and Engels.

Kluchert sets forth his work in four main sections. In the first, he traces the evolution of the thought of Marx and Engels until 1846. The second section is devoted to the "mechanical" connection that they saw between economic progress and sociopolitical movements in which the development of the productive forces provided history with its principal dynamic. In the third section Kluchert describes the conversion of Marx and Engels to the view that the revolutionary movement had its own dynamic—an inner dialectical logic that sprang from the changing relationship of classes and parties and that did not significantly depend on the level of the forces of production. The fourth and final section is devoted



to an analysis of the effect on Marx and Engels, by the end of 1848, of the realization that the revolution was at an end: for Engels, the result was a reemphasis of the determining role of the productive forces, as in *The Peasant War in Germany*; Marx also returned to economic determinism but in a more subtle form in which the forces of production and the class struggle were mediated through complex relations of production, as in *The Eighteenth Brumaire*. According to Kluchert, each of these historical models corresponded to a different political perspective: respectively, support for the most radical wing of the bourgeoisie, the proclamation of permanent revolution, and organization and enlightenment of the working class in a longer term revolutionary perspective.

All this is possibly a little too schematic—the political proposals of the *Communist Manifesto* scarcely fit the first model—and Kluchert is chary of drawing any general conclusions about the historiography of Marx and Engels. But the book remains a thorough, detailed, and well-rounded contribution to the study of their political and historical activity.

DAVID MCLELLAN  
*University of Kent at Canterbury*

STANLEY LIEBERSON. *Making It Count: The Improvement of Social Research and Theory*. Berkeley and Los Angeles: University of California Press. 1985. Pp. xiv, 257. \$19.95.

All modes of scholarly inquiry make assumptions about the nature of reality. In this insightful volume Stanley Lieberman, a sociologist best known to historians for his comparative study of the economic progress of American ethnic and racial groups during the twentieth century, criticizes the assumptions that social data are essentially similar to those generated in scientific experiments and that the tool fabricated to analyze these experiments, statistics, provides a procedure sufficient to comprehend and organize the underlying realities.

This inquiry is important and stimulating because Lieberman's critique is firmly grounded in this tradition of social science. Lieberman focuses on the use and limitations of control variables in social research. The statement that B causes A implies that variation in B, the independent variable, is associated with variation in A, the dependent variable. It is necessary to show, however, that other influences, on the outcome (C...Z) do not confound the relationship between B and A. Drawing on examples from the sociology of education, Lieberman effectively argues that the inclusion of these control variables in the analysis often does not eliminate possible nonrandom assignment of cases to one of

the categories of variable B. Society thus rarely presents natural experiments for the social scientist to analyze.

Many of the tasks social scientists undertake are undoable, Lieberman contends, because they are inherently or theoretically impossible, premature, or overly complicated. Much of what historians write about falls into one or more of these categories of impracticable projects. Still, his discussion of the undoable is valuable. Although few historians claim that singular events are determined, Lieberman concretely shows how probability is relevant to the claim of inevitability. He also insightfully develops the concept of variance—why attempting to explain it completely is often misguided and why its absence may be more substantively important than its presence.

Also of considerable interest to historians is Lieberman's discussion of the important conceptual distinction between symmetrical and asymmetrical relationships. Statistical procedures assume symmetry, but reality may be asymmetrical; the incidence of a behavior may change, for example, if income increases but not if it decreases. Causation can realistically be assessed, he argues, only with longitudinal data. This is an unusual position for a sociologist to take. Lieberman forgets that time, even more than specific social context, is contaminated by a variety of effects and complicated by unmeasured or unmeasurable variables. The results of temporal analyses cannot be generalized to other intervals of time. All the analyst can do is to describe accurately, which is, as Lieberman notes, no trivial achievement.

This book should be read and pondered by historians who deal with quantitative data. Additionally, historians whose efforts involve the consideration of a multiplicity of possible influences on behavior will find much of interest in the book. The numerical version of multivariate analysis is, after all, only one representation of a general problem in the logic of interpretation.

DANIEL SCOTT SMITH  
*University of Illinois,  
Chicago*

ELI SAGAN. *At the Dawn of Tyranny: The Origins of Individualism, Political Oppression, and the State*. New York: Alfred A. Knopf. 1985. Pp. xxiii, 420. \$22.95.

Eli Sagan deals with no less a problem than the origins of the state. Sagan's book is part of the growing literature that seeks to illuminate the murky but crucial transition from prehistoric kinship societies to early archaic civilizations. Sagan makes the reasonable assumption that a direct leap from clan organization and village culture to the elaborate monarchies of Mesopotamia, Egypt. In-

dia, China, and other archaic states is unlikely. He postulates a multileveled intermediate stage he calls "complex society," comprising both chieftainships and simple early forms of monarchy—political structures that lacked the elaboration of the archaic state but were politically much more complex than the kinship societies that preceded them. Sagan then proceeds to analyze three such intermediate polities—Buganda, Tahiti, and Hawaii in the eighteenth and nineteenth centuries—in search of the driving forces behind this transition from the amorphous authority of kinship society to the relatively rigorous authoritarianism of the archaic monarchy.

The origins of the state have been traced to a variety of forces and circumstances, including population pressure, private property, social inequality, war making and military conquest, redistribution of the social product by charismatic "big men," despotic control of rivers, an "urban revolution," and various forms of social and economic evolution from the hunter-gatherer band to the political state. Sagan finds his key to this vital transformation in psychoanalytic theories of developmental psychology. The evolution of the state, he asserts, recapitulates the stages of individual psychological development: "The development of the psyche is the paradigm for the development of culture and society" (p. 364).

Sagan's earlier works include studies of cannibalism as a primitive cultural form and of violence in ancient Greek culture. His accounts of the three transitional societies he examines here are clear, largely free of jargon, and eminently readable. He bases his descriptions on first-hand reports, both by Western explorers and missionaries and by indigenous chroniclers. Both style and content seem to aim at a wide readership.

For historians, the book's primary defects may include impressionism, undue dependence on historical parallels, and the book's psychohistorical core. Many historians will not be convinced by impressionistic characterizations of entire societies as "confident" or "anxiety ridden," supported largely by anecdotal evidence or by descriptions of ritual practices. The author's comparisons between these African and Polynesian societies and familiar institutions, events, and personalities from Western history too often seem to be rather casually drawn. The psychoanalytical theories of Anna Freud and Margaret Mahler, finally, will not seem a solid foundation to those who are not convinced by Freudian accounts of psychic drives and mechanisms or of their unmediated relevance to the evolution of human institutions. Nevertheless, the book deals with such a crucial problem and approaches it so thoughtfully that, like such earlier tours de force as

Marvin Harris's *Cannibals and Kings* (1977), it deserves serious consideration by historians.

ANTHONY ESLER

*College of William and Mary*

JAROSLAV PELIKAN. *Jesus through the Centuries: His Place in the History of Culture*. New Haven: Yale University Press. 1985. Pp. xvi, 270. \$22.50.

*Jesus through the Centuries* is the text of a set of eighteen lectures given at Yale on what Jaroslav Pelikan describes as the "image of Christ" as it has been viewed through the Christian era. "Image" means, in principle, what Christ has meant not to the church or to those who have hoped to be saved by him but to the world at large. Pelikan thus devotes roughly one lecture to each century, and I think the book will be found to cohere rather better as a history if this simple thought is borne in mind. (Thus, one should read chapter 9, on monks, between chapters 6, on Augustine and the Incarnation, and 7, on images and iconoclasm.) This would make it a book rather on the lines of Kenneth Clark's *Civilisation* (1969), except for a relative lack of visual aids; but there might be something to be said for that.

As Clark had with "civilization," Pelikan has one or two problems with "culture": he says that he wants to use the word to mean something more popular or anthropological than high culture, on the grounds that this would be inappropriate for a philistine such as Jesus. On the other hand, if one excludes popular or anthropological Christianity, as Pelikan on the whole does (no Sacred Heart, for example), only high culture remains, since Jesus will probably not mean much to a non-Christian philistine. The images of Jesus he records are actually rather learned ones, and one conception of him that could be argued to have had a rather considerable influence on non-Christians (the Child Jesus, the Holy Family) is scarcely mentioned.

Pelikan's historical Jesus is also a bit refined because of the temptation to preach a course of lay sermons to liberal Americans while avoiding saying anything about Christianity that they will dislike. So St. Augustine is not really against sex; anti-Semitism has nothing to do with any genuine image of Christ; the concept of the Logos founds the European intellectual tradition; the Reformation does culture no harm; Jesus is a liberator, model of Lincoln, Tolstoy, Gandhi, and Martin Luther King, though not exactly of revolutionary praxis and not, or not yet, of feminism. Sometimes, in his concern to show that Jesus is no longer the prisoner of a declining church but belongs to the world, Pelikan sounds awfully like those Emersons and Renans on whose identification of Jesus with uplifting waffle he has a

lively chapter. Apart from them, I thought the three spectators whose images came across most forcibly were Vergil, Anselm, and Thomas Jefferson. Jefferson's "abridgment of the New Testament for the use of the Indians, unembarrassed with matters of fact or faith beyond the level of their comprehensions" is a welcome addition to knowledge (p. 190). In short, this is a readable, learned, and humane book, but, personally, I think Jesus is rather more of a problem than Pelikan makes out.

JOHN BOSSY  
University of York

D. G. PAZ. *The Priesthoods and Apostasies of Pierce Connelly: A Study of Victorian Conversion and Anticatholicism*. Studies in American Religion, number 18). Lewiston, N.Y.: Edwin Mellen. 1986. Pp. xxiii, 422.

In this book D. G. Paz expands his long-term interest in anti-Catholicism into a transatlantic study of clerical conversions (or "apostasies"). His protagonist, Pierce Connelly, may hold the record for conversions and reconversions.

Reared in Philadelphia as a Presbyterian, Connelly became an Episcopalian while an undergraduate and was later ordained an Episcopalian priest. In 1831 he married Cornelia Peacock and became rector of a parish in Natchez, Mississippi. Here, in isolation, the young high ("Hobartian") churchman, provoked by evangelical anti-Catholicism and obsessed by the question of church authority, decided to convert to Roman Catholicism in 1836 and took Cornelia with (indeed, ahead of) him. Paz notes some ambiguities in this conversion, but the main problem was that Connelly still had a priestly vocation in a church that disallowed married clergy. In Rome he found the solution by having Cornelia enter a convent, which allowed him to be ordained a Roman Catholic priest in 1845; both then went to England to pursue their new careers. But in the process Cornelia had discovered an independent personality and a vocation; she was to become the foundress of an order, a nominee for canonization, and the subject of biographies. She denied Connelly the visitation rights to which he thought himself entitled as her husband. This, together with tactless treatment by Bishop (later Cardinal) Nicholas Wiseman, led him to place himself in the hands of virulent anti-Catholics. In a notorious case, Connelly sued for restitution of conjugal rights, eventually losing on appeal. He not only left the Roman church but, like other former priests whom Paz discusses, he joined the underworld of anti-Catholic pamphleteers. Connelly eventually settled in Florence, where, on the strength of his original ordination, he ended his days as minister of the American Episco-

pal Chapel from 1867 to 1883, a dry low churchman with "a sterile and arid faith" (p. 227).

Paz examines Connelly as a case study in conversion and anti-Catholicism and as an example of the interconnectedness of American and British religious history. He makes many valuable points in these regards, although one case study of an atypical denomination is not sufficient to prove the theory of American religious uniqueness "untenable" (p. 231). But his case study is ultimately a biography, and Paz goes beyond his modest stated themes to engage in an essay in spirituality, a genre rarely attempted by lay professional historians. This deserves some respect, being informed by a true feeling for the spiritual experiences he describes, even if Paz sometimes shows too much trust that such spirituality can be documented and analyzed. He may not be able to prove that Connelly "loved the Church rather more than he loved God" (p. 224), but it is remarkable that he asks the question, which raises this sad story of a wasted life to a level beyond its subject.

JOSEF L. ALTHOLZ  
University of Minnesota

JONATHAN R. ADELMAN. *Revolution, Armies, and War: A Political History*. Boulder, Colo.: Lynne Rienner. 1985. Pp. xii, 268. \$28.50.

Armies, according to Jonathan R. Adelman, are microcosms of society, and the study of revolutionary armies and their evolution over time is revealing of the impact of revolution on society. Much of this book builds on the pioneering efforts of Katherine Chorley, the seminal study of Theda Skocpol, and the valuable work of other scholars. It examines the victories of the New Model Army, Napoleon's army, Russia's Red Army, and China's People's Liberation Army, which were won by states that had been weak in prerevolutionary days. Ordinarily, such transformations come about after a lengthy historical process, but in revolutionary England, France, Russia, and China they occurred rapidly and helped lift their countries from relatively inferior positions to ones of strength.

Adelman states that "the fate of all major revolutions has ultimately been decided on the battlefield" (p. 4). Although in part true, the statement contains parochial overtones since some scholars contend that political or economic forces are as important as military ones. This illustrates a problem with comparative studies of revolutions: since revolutions are varied and distinctive societal phenomena, generalizations are flawed almost by definition. This does not deter Adelman, and it should be but a modest caveat to this otherwise fine study.

These pages reflect little new research, but that is not expected in a comparative study. What is ex-

pected—and what we get—are fresh points of view and a new look at what are otherwise separate cases. In the process a number of myths about the military are refuted as Adelman demonstrates the myriad ways revolutions enhance the capacity of new central governments to wage wars and exercise power.

Single chapters for England and France and several chapters for Russia and China (the book's major focus and the subject of Adelman's earlier work) effectively scrutinize old regimes and new revolutionary states to explain how revolution transformed the armed forces and built state power. Only a token amount of comparison is made between the different revolutions prior to the concluding chapter. When such references sprout on Adelman's historical landscape the overall picture is given greater depth and precision. More evident, and thus of greater value, are the comparisons between pre- and postrevolutionary military, foreign, and domestic policies and actions. From this he correctly concludes: "Revolutions . . . are pathways to modernity and revolutionary armies are important actors in that process" (p. 209).

The book contains minor inconsistencies in punctuation and typographical errors that blemish an otherwise well constructed, competently written book. For example, the "Thirty Years' War" and "Seven Years' War" appear with and without apostrophes; commas are not used consistently following adverbs such as "thus"; and the War of the Austrian Succession began in 1740, not 1741 (p. 37).

Adelman observes that his revolutions occurred in states that were rich in resources. Without this consideration revolution can easily be made to appear a panacea. This also suggests that the author's emphasis on the pivotal role of armies may be somewhat overstated. Thus, as important and stimulating as his book is, its long-term value may be in launching a re-examination of the causes of success of revolutionary states.

ROBERT BLACKKEY  
California State University,  
San Bernardino

#### ANCIENT

O. KIMBALL ARMAYOR. *Herodotus' Autopsy of the Fayoum: Lake Moeris and the Labyrinth of Egypt*. Amsterdam: J. C. Gieben. 1985. Pp. xiv, 160. f. 65.

Nothing in Herodotus's *Histories* has provided such grounds for attacking his veracity as his description of Egypt. The attacks generally proceed on two fronts: it is argued, first, that his travels were less extensive than he claimed, and second, that he plagiarized from his predecessor, Hecataeus of

Miletus, who visited Egypt before him and is the only prose writer whom Herodotus names as a source. O. Kimball Armayor's book attacks on still another front. He looks specifically at Herodotus's description of the Lake of Moeris and the Labyrinth (2. 148–50) and argues that Herodotus misunderstood an "ironic" account of Hecataeus, who was not describing a real lake or labyrinth but a Pythagorean concept: Moeris and the Labyrinth and probably much else in the *Histories* belong to the Egypt of the Greek imagination, not the Egypt of reality. Herodotus, we are asked to believe, was a simple soul who mistook Hecataeus's humorous intent.

Herodotus described the "Lake of Moeris" in the *Faiyum mudiriyyah* as a body of water 3,600 stades in circumference, excavated by the pharaoh Moeris. Moeris is arguably Amenemhet III, whose name may have been attached to the lake by a misunderstanding: the Egyptians called it *ta-henet-en-Mi-wir*, that is, the "lake of *Mi-wir*," and *Mi-wir* was Gurob, a town close to the Canal of Joseph, which feeds Nile water into the Fayoum. But to Greek ears *Mi-wir* sounded very like Moeris, which, it appears, is a corruption of Amenemhet's throne name Ny-maat-re, and the tradition that Amenemhet had been active in the Fayoum was strong and accurate. So that the "lake of *Mi-wir*" became the lake of the pharaoh Moeris is understandable. But Herodotus's great lake no longer exists. All that remains is the small, salty Birket Qarun, forty-five meters below sea level, in the northwest corner of the Fayoum.

In 1929 Gertrude Caton-Thompson and E. W. Gardner argued in the *Geographical Journal* that Herodotus's lake did not exist in his time either and that he had been deceived by a mirage. Caton-Thompson and Gardner followed up this article in 1934 with *The Desert Fayum*, which is a careful study of the geology and prehistory of the area and serves as a cornerstone for Armayor's thesis. But his enthusiasm has blunted his critical faculties. The views of Caton-Thompson and Gardner on Lake Moeris have been abandoned. One item of evidence on which they relied, the dates of the Fayoum A and B cultures, has collapsed as a result of new radiocarbon dates. Moreover, later geological research has not supported them. The latest, not mentioned by Armayor, is a survey in 1968–69 carried out by a team from Southern Methodist University cooperating with the Polish Academy of Science and the Geological Survey of Egypt, which found evidence for a lake covering close to twenty-two hundred square kilometers in the Old Kingdom period. Armayor also does not mention Barbara Bell's work on the Middle Kingdom climate, which presents evidence that Amenemhet used the Fayoum as a run-off basin to counteract the effects of high floods recorded for the period. Herodotus exaggerated



the lake's size, and he was wrong to claim it was excavated. But we cannot doubt that he saw it.

Armayer is no more persuasive on the Labyrinth. Not much of it remains now, although more existed in the mid-nineteenth century when K. R. Lepsius dug there. Armayer sheds no light on questions concerning its date and structure. In fact, it is hard to find much to recommend in this book. It fails to prove its point, and it is not a reliable guide to recent research in the Fayoum area.

J. A. S. EVANS

*University of British Columbia*

TH. RIZAKIS and G. TOURATSOGLOU. *Epigraphes Anō Makedonias (Elimeia, Eordaia, Southern Lynkestis, Orestis)*. [Inscriptions of Upper Macedonia]. Volume 1, *Catalog of Inscriptions*. Athens: Archaeological Receipts Fund, with the cooperation of the Ministry of Culture and the National Research Foundation. 1983. Pp. 253. dr. 3,000.

The systematic collection and publication of the inscriptions of ancient Macedonia have been hindered by the region's geographical isolation, difficult terrain, turbulent modern history, and slow development of archaeological surveys and excavations. Early European travelers published some careful and detailed descriptions of Macedonian topography and antiquities, but their outlook was often antiquarian. The survey of the ancient remains was casual until after the incorporation of much of ancient Macedonia into the modern Greek state following the Balkan Wars.

With Macedonia now under the aegis of the Greek archaeological service and the commencement of excavation in the 1920s and 1930s, the recovery of antiquity began. It fell to Charles F. Edson of the University of Wisconsin to start collecting and editing the epigraphical materials. This project occupied Edson from 1937 until the publication of part of the documentary corpus (more than one thousand inscriptions from the region of Thessaloniki) in *Inscriptiones Graecae* X.II.1 (1972)—a brilliant effort, but only a piece of what Edson and others had collected.

The deficiency of an organized assembly of the epigraphic evidence is now being corrected by a project jointly sponsored by the Greek Ministry of Culture and the National Research Foundation: a series of publications that will systematically present the ancient inscriptions of Macedonia. This first volume offers the texts (a companion volume of commentary will follow) from "Upper" Macedonia, a region including the rugged mountain country west of Mount Vermion up to the frontiers of Epiros and Albania, from the Yugoslav border to Thessaly. The Upper (or western) Macedonian can-

tons surveyed in this volume are Elimeia, Eordaia, Orestis, and southern Lynkestis. Unfortunately, modern politics prevented the inclusion of the material from northern Lynkestis, an important part of ancient Macedonia from the time of Philip II, which now lies within Yugoslavia. Nine inscriptions from the fifth western canton of Tymphaia are catalogued among the finds from Elimeia. As far as we know, there were no major cities in the region in antiquity, although the evidence testifies to widespread settlement and the maintenance of vital road links between the Adriatic and Aegean.

Editors Th. Rizakis and G. Touratsoglou present 225 inscriptions, arranged geographically by canton and further subdivided by type and chronology. Each entry consists of a description of the object's discovery (many were stones reused in later buildings), dimensions, bibliography, text (with scholia), date, and an assessment of major problems and parallels. The editors should be congratulated for this clear and useful plan. Each inscription is accompanied by a photograph (most of which are new) or drawing. Some fifteen indexes and concordances provide cross references to texts, bibliography, and museum or collection numbers. A map of the western cantons shows the distribution of finds.

Chronologically, the material ranges from an early fifth-century B.C. phiale dedicated to Athena to some mosaics of the sixth century after Christ. More than half come from the Roman second and third centuries. Most are familiar categories, such as dedications, epistles, honors, grants on plain and relief stelai and statue bases. Some more modest types are also included, such as a series of roof tiles stamped with the name of Philip II, materials as important for an understanding of the power of the Macedonian monarchy as are formal decrees. All but four inscriptions are in Greek. Forty-seven are here published for the first time.

The inscriptions are too few to permit any valid statistical analysis. We possess only what has survived and been recovered, and we have little basis for estimating what originally existed. Perhaps the future publication of inscriptions from the more heavily settled regions of ancient Macedonia will permit the possibility of a sound evaluation.

The systematic presentation of these western Macedonian inscriptions in such a well-produced volume is useful. One hopes that subsequent volumes will maintain the same high standard.

EUGENE N. BORZA

*Pennsylvania State University*

*American School of Classical Studies, Athens*

PETER ØRSTED. *Roman Imperial Economy and Romanization: A Study in Roman Imperial Administration and the Public Lease System in the Danubian Prov-*

*inces from the First to the Third Century A.D.* Copenhagen: Museum Tusculanum. 1985. Pp. 415. Cloth KR 360, paper KR 280.

In the expansive introductory part of his work Peter Ørsted essays a grand theory, replete with "sociological models," concerning the economic and political development of the Roman empire. The thesis holds that the urban centers that so typified the structure of the empire grew out of the economic interests that emerged between the private "capital" of individuals (or rather their *familiae*) and the central state. In short, Ørsted contends, the operation of the leasing system for the collection of imperial tribute and the provision of goods and services for the government, and the consequent influx of imperial coinage, fueled the growth of local town centers in the empire. Urbanization was stimulated by profits from the leases held by the local men who bid for government contracts. The leasing system was therefore the proximate cause of the rise of new urban centers in the empire (at least in the western part of the empire, since the theory, the author is forced to admit, explains little about developments in the eastern half). The atrophy of this system and the subsequent direct intervention of the government in the economy led to the decline and disappearance of the cities and to the fragmentation of the local urban economies into those of smaller households (*oikoi*), whose economies were characterized by unfree labor. Therefore, according to Ørsted, the leasing system determined the rise and fall of the cities that were the core units of the "Romanization" of the empire (p. 53).

The title and the first chapter promise a real theoretical treat, but, on beginning the lengthy second part of the book on the nature of the leasing system itself, the reader realizes that this is a traditional slog through "the evidence," replete with much epigraphical guesswork and juristic minutiae. Readers will learn nothing new here; indeed, they will encounter much that is distinctly old. In the absence of evidence relevant to the operation of the public leasing system at the local level, meaningful analysis of the economic and social dimensions of the letting of government contracts is impossible. Instead, one finds extended, and sometimes terribly recondite, discussions of the precise technical jargon and legal status of public contracts and of the parties involved (for example, page 126 on the early development of the leasing system). In all of this Ørsted never advances very far beyond positions reached by nineteenth-century scholarship. For example, he stridently maintains the theory of an absolute division between a "senatorial" or public system of leases, which, he claims, was always subject to "public law," and the emperor's personal system, which was under the aegis of "private law." In attempting to

apply this division with some consistency, Ørsted is compelled to disregard the consensus of current scholarship, which has demonstrated beyond reasonable doubt that such a strict legal or constitutional division of state power, authority, and resources was never operative in the empire. Undaunted, Ørsted continues to insist on an outmoded Mommsenian dyarchy, a conflict between emperor and senators, as central to an understanding of the economic operation of the empire, despite the continual problems that this causes his analysis (pp. 206, 219). Furthermore, the hair-splitting analysis of legal terminology on which most of the argument reposes depends on a fixity in the significance of words (such as *emere*, "to buy" and *vendere*, "to sell") that Ørsted himself repeatedly demonstrates to have been fluid in meaning (for example, contrast his statements on pages 68–70 with those on page 89).

Having established the theoretical basis for his argument in the first half of the book, Ørsted then advances to test his model. Apparently undisturbed by the lack of written evidence on leasing itself, Ørsted selects Noricum and Illyricum as the test region for his theory. Now, in the ancient world, for which literary evidence on economic matters is never very good, in remote regions such as Noricum it almost vanishes. So, instead of any possibility of a genuine test, we get further guesswork. The very cities whose rise and decline Ørsted wishes to explain are so badly documented that we cannot even be sure of their formal municipal status. Ørsted asserts, as he must for his theory, that they were *municipia* (p. 197), but other scholars, such as Brigitte Galsterer-Kröll, have presented a decisive case to the contrary. Hence Ørsted's vaunted model that links the leasing system and the operation of the iron mines of Noricum ("Test Case A") with the rise of Roman-style cities in the province reposes on little more than speculation. In the face of a virtual absence of Norican data relevant to the problem, Ørsted is compelled to plunder evidence on mines in Spain and Dacia. The Norican evidence, reduced to five meager inscriptions, does not in fact constitute any sort of acceptable historical test. The same indefensible method mars "Test Case B" (the collection of "customs duties" in Illyricum).

No doubt, a good argument can be made that the taking of government contracts was part of the process of the economic integration of local elites into the central government of the empire and that the letting of public contracts had economic consequences for the development of new urban centers. But Ørsted overstates his case to the point of absurdity and in the process repeatedly boxes himself into untenable positions. In the general sense in which his model might be valid, it is either banal, untestable, or both. In final judgment, one must suspect



that the ominous and lengthy subtitle gives a truer indication of the contents of this volume than does its pretentious leader. The detailed studies on the mines and "customs" should have been published as articles to serve the cognoscenti of matters Norican and Illyrican. But historians of Rome who eagerly look forward to the sort of analysis promised by Ørsted's title will be disappointed.

BRENT D. SHAW  
University of Lethbridge

## MEDIEVAL

E. A. THOMPSON. *Who Was Saint Patrick?* New York: St. Martin's. 1985. Pp. xv, 190. \$21.95.

Recent scholarship has established that the only reliable sources for the life of St. Patrick are his own writings, and these have been reduced to the short *Confession* and the *Letter to the Soldiers of Coroticus*. This enables the accretions derived from later annals and legends to be cut away but still leaves room for almost unlimited controversy over the interpretation of the texts. Patrick grew up speaking the vernacular Latin current in late Roman Britain, and his formal education was cut short when, at the age of sixteen, he was captured by slave raiders and carried off to Ireland. His "passionate stumbling sentences" convey the strength of his feeling but do not always express his meaning clearly. Interpretation is difficult even for skilled Latin scholars. Consequently, when E. A. Thompson declared his intention to write a book about St. Patrick that "could be read and understood by persons who knew no Latin" and was based on sound sources, he stated only half the problem (p. xi). Unhappily, although he is a distinguished classical scholar with an intimate knowledge of the late Roman world, he has not wholly overcome the difficulties either of interpretation or of presentation.

Thompson's intended audience is not entirely clear. Perhaps he has set his sights too low. At times he seems to address readers who are as ignorant of history as of Latin. Many pages are devoted to repetitive and hectoring dismissal of the type of absurd interpretation that might be put on a literal modern translation of Patrick's fifth-century Latin. Yet the untrained reader is offered only patchy and inadequate information about the world in which Patrick lived. For example, Thompson never clearly explains the duties of a fifth-century bishop or gives any indication of what parishes then were. Again, in reconstructing events, Thompson gives overmuch weight to silence; although nothing in Patrick's own words justifies a theory that he spent time at

Auxerre, this does not prove that he never went there. Yet Thompson also makes some unwarranted assumptions. He suggests that Patrick expressed distress at the inadequacy of his Latin because his interrupted education cut him off from "the landed gentry into which he was born" and that Patrick thus manifested "crude class-consciousness" or a "chip on his shoulder" (p. 146). This view overlooks the possibility that Patrick, struggling to express his conviction that in spite of his unworthiness God had chosen him rather than apparently better men for missionary work among the pagan Irish, was painfully aware of the inadequacy of his halting Latin to convey his meaning. Surely this may have been genuine humility.

At least, for readers who wish to go on to deeper study of St. Patrick, the concluding section on further reading is critical and helpful. But, regrettably, the treacherous bog of Patrick studies, which even a Bollandist can scarcely traverse dry-shod, has claimed yet another illustrious victim.

MARJORIE CHIBNALL  
Clare Hall  
University of Cambridge

BEVERLY KENNEDY. *Knighthood in the Morte Darthur*. (Arthurian Studies, number 11.) Cambridge, England: D.S. Brewer; distributed by Boydell and Brewer, Dover, N.H. 1985. Pp. ix, 394. \$47.50.

NORMA LORRE GOODRICH. *King Arthur*. New York: Franklin Watts. 1986. Pp. ix, 406. \$21.95.

King Arthur has inspired much wishful thinking. Winston Churchill's statement that "Arthur, if he did not live, should have" (*History of the English-Speaking Peoples* [vol. 1, p. 58]) prefaces Norma Lorre Goodrich's attempt to recover the Arthur of history. Beverly Kennedy's subject is not Arthur himself but rather his function as a model of knightly virtue in literature and legend.

A close reading of Thomas Malory's *Morte Darthur*, its analogues, and its sources, Kennedy's work aims at constructing a "typology" of knighthood. Basing her approach loosely on the work of Clifford Geertz and Paul Ricoeur, Kennedy claims that Malory set out to explore a three-fold concept of knighthood: heroic knighthood, true knighthood, and worshipful knighthood. In six chapters she explicates the framework for this typology and analyzes Malory's exploration of feudal, courtly, and religious virtues in the careers of Arthur, Gawain, and Lancelot.

Although Kennedy's introduction is tantalizing, her presentation of methodology seems inadequate. Kennedy calls hers a "phenomenological" approach because it seeks to enlarge "the interpreter's field of

vision to take in the entire world of the text," a technique particularly suited to Malory, in whose world she sees three different perceptual perspectives (p. 2). Kennedy wisely concedes that "it does not matter whether Sir Thomas Malory was consciously aware of each of these three different points of view within the world disclosed by his text" (pp. 3, 328)—a fundamental assertion of hermeneutics—but she also asserts that Malory consciously intended to exemplify those perspectives (as with "true knight-hood," pp. 92–3, 97). This recourse to intentionality undercuts her approach; an analysis of Malory's tripartite discussion of knightly conduct must stand on its own merits, independent of attempts to validate it as the author's intention. Kennedy also struggles to transform her elaborate thematic triads into evidence for the unity of Malory's text as found in the Winchester manuscript, as if that organicist goal were the ultimate objective of literary criticism (pp. 7, 328–9).

Kennedy examines knighthood in various medieval sources; her subject is the historiography of Malory, Caxton, and others who retold Arthur's story. She writes about texts, not about knighthood as a social institution. But her well-informed and clearly written work moves confidently between literary and cultural evidence and should be read alongside Maurice Keen's *Chivalry* (1984) and other studies that analyze the survival of chivalry outside literature and the separation of literary evidence from social reality.

Goodrich also writes about the reshaping of Arthurian texts, but her subject is history rather than historiography; her detailed and frequently laborious investigation is designed "to offer the first historical proof of the existence of King Arthur" (p. 3)—a daring claim, attractive to those convinced that an Arthur can be recovered from myth, but unlikely to impress skeptics. The rambling structure includes chapters on geography, Geoffrey of Monmouth, Guinevere, the Grail Kings, and, of course, Arthur's passing. Readers will find the generous antiquarian lore at least interesting, and travelers may find the geographical details useful. The notes are vague, and commentary in the partially annotated bibliography is sometimes gratuitously sour.

Goodrich employs approaches more impressionistic than scholarly (for example, descriptions of locales important to Arthurian legends). Although she refers to new methodologies and "new disciplines" (pp. 9, 28, 45), Goodrich means no more than paleography, stylistic analysis, and traditional comparative techniques. And, in spite of these testimonials to modern tools, her own methodology is quaintly described as a "word-by-word rereading of the Arthurian manuscripts" of "quality" (p. 33). "Certain texts were discarded along the way as being too corrupt, poorly written, and generally unreli-

able," Goodrich writes. "In those cases the authors were found wanting in intelligence and talent" (p. 35). No doubt they lacked "good taste" as well. Elsewhere Goodrich writes, "The more intelligent authors employed in their treatments all the resources proper to great literature" and asks, "But what does that mean to us" (p. 7)? Her unexpected answer is that such resources—the mythological traditions informing "highly imaginative prose"—actually reinforce the historical validity of texts. The opposite conclusion seems more plausible.

Readers of either book may wonder why Goodrich and Kennedy merely invoke rather than evaluate the historical and literary concepts—cultural tradition, great literature, thematic unity—informing their judgments. Neither book confronts its theoretical assumptions directly, but Kennedy's study is ambitious and, finally, more revealing of Arthur's importance than the industrious efforts of Goodrich to reinvent a Christian king in his image.

ALLEN J. FRANTZEN  
Loyola University  
Chicago, Illinois

MARJORIE CHIBNALL. *Anglo-Norman England, 1066–1166*. New York: Basil Blackwell. 1986. Pp. 240. \$39.95.

Marjorie Chibnall states in her introduction that "in writing this book my object has been to ask new questions rather than to rehearse arguments for and against the ancient theories; and to try to relate the recent findings of historians in specialized fields to the general course of English development" (p. 3). Both purposes are fulfilled, which is no mean achievement in a field acknowledged to have been, for some centuries, "rewritten in every generation" (p. 1).

Chibnall is a leading scholar in Anglo-Norman studies, but this publication is strong not as a work of scholarly detail but as a wide-ranging survey written with clarity and firmness to communicate a convincing perspective. It will be especially appreciated by teachers of the subject, who will find in it clear factual exposition, balanced evaluation of the classic interpretations that still survive modern criticism, and a valuable guide to recent research over an impressively wide range of specializations. The footnotes and bibliography are particularly useful pointers to key writings old and new.

The book is divided into three parts: "Conquest and Settlement," "Wealth and Government," and "Law and Society." Part 1 deals with the first phases of conquest, the moving frontiers with Wales and the north, and settlement and succession throughout the whole period. Chibnall considers that "the first phase of conquest was over by the summer of

1070, and the more permanent pattern of Norman settlement began to emerge" (p. 19); the later phase secured enfeoffments and converted many acquisitions into patrimonies and was still "far from complete even in 1086" (p. 14). After the Conqueror's death "his sons seem to have resolved that the divided inheritance should ultimately be reunited" (p. 57); this situation had political consequences for the remainder of the period. Part 2, reversing the order of its title, deals first with the government and only second with the wealth of England. Chibnall believes that in this period administrative kingship arose in England and that "the central core of government was rapidly becoming both literate and numerate on both sides of the Channel" (p. 105). Domesday Book belongs to this administrative setting: "Behind it lay the experience and records amassed in the course of government" (p. 106), and the Norman kings' remarkably precocious methods of centralized administration are examined. In part 3 Chibnall traces the developments producing the English common law, reviews serfdom, villeinage, and the manorial courts, and surveys canon law and the church courts. A much less technical chapter, "Normans and English," rounds off the book by considering racial, linguistic, and artistic assimilation; the epilogue underlines that society and institutions in England were more profoundly affected by the conquest of 1066 than by incorporation into the Angevin empire in 1154.

A characteristic of recent Anglo-Norman studies has been the shedding of insularity: "There is a far keener appreciation of the relevance of European social developments to changes taking place in England and Normandy both before and after the conquest" (pp. 1-2). The focus on interaction between English and Norman traditions and institutions after 1066 is clearly justified in discussing such subjects as knightly service, ecclesiastical vassalage, royal administration, manorialization, secular and canon law, and what may be defined as Anglo-Norman culture.

Although the index disappoints, the book is well produced and includes nine maps and two genealogical appendixes.

HELEN M. JEWELL  
University of Liverpool

DAVID CROUCH. *The Beaumont Twins: The Roots and Branches of Power in the Twelfth Century*. (Cambridge Studies in Medieval Life and Thought, fourth series, number 1.) New York: Cambridge University Press. 1986. Pp. xxi, 242. \$39.50.

David Crouch has managed a doubly impressive feat: a study that is as genuinely informed (and informative) about Normandy as about Norman

England and life-and-times biographies of two members of that cross-Channel society. The book focuses on the parallel and interlocked careers of the twin sons of Robert de Beaumont, Waleran (1104-64), count of Meulan and earl of Worcester, and Robert (1104-68), earl of Leicester. In the political narrative, which occupies the first hundred pages of text, as well as in the general tone of the entire work, Crouch nicely balances his judgments. Robert emerges as the more able and successful politician, because he was more level-headed and consistent about options and choices. Advancement under Henry I, then loyalty to King Stephen, then a single and expertly timed switch to Henry Plantagenet enabled him to expand his inheritance and to have his abilities, more administrative than military, rewarded by service as justiciar in the new Angevin regime. In contrast, Waleran is seen as less decisive and hard-headed, indeed almost feckless, yet his broader cultural interests, both spiritual and chivalric, and the spectacular difficulties he caused himself prompt a more sympathetic discourse than does Robert's political acumen and enable Crouch to sketch Waleran's personality more sharply.

The second half of the book is devoted to a descriptive analysis of the brothers' lordships on both sides of the Channel: the honor as unit and as concept, administrative personnel and institutions, types and rates of income, and ecclesiastical patronage. The details are assembled on the basis of careful charter scholarship. They amplify and extend, rather than alter, the general picture built from earlier studies and editions by scholars such as Sir Frank Stenton, Sidney Painter, John Le Patourel, R. B. Patterson, and W. L. Wightman. The book is especially valuable for its genuine cross-Channel perspective. In that sense, it is closest to, but fuller and more balanced than, Wightman's *The Lacy Family in England and Normandy, 1066-1194* (1966), and it is informed much more by the spirit of Le Patourel than by the tradition of Stenton.

As a final historiographical note, the book is the first title in the new, fourth, series of Cambridge Studies in Medieval Life and Thought, under the general editorship of J. C. Holt. The previous series were justly renowned for their high standards of important scholarship; Crouch has gotten the new series off to an equally auspicious start.

MICHAEL ALTSCHUL  
Case Western Reserve University

R. H. BRITNELL. *Growth and Decline in Colchester, 1300-1525*. New York: Cambridge University Press. 1986. Pp. xvi, 304. \$47.50.

In this new addition to the growing literature on the late medieval urban crisis, R. H. Britnell has picked

the town of Colchester, in Essex, because it "implies a convenient standard of comparison with other towns" (p. 5). He eschews *histoire totale* and writes instead an economic and institutional history, with special focus on the overseas trade. The history is divided into three epochs. In the first, 1300 to 1349, Colchester was a small market center of three to four thousand folk. The town conducted little long-distance trade and limited textile production. Rather, the path to prosperity came from control of local food markets. The burgesses were autonomous. They elected officers, supervised courts, and answered only to the king. But the lack of specialization limited their wealth, and they had little influence beyond the city itself.

The second epoch, 1350 to 1414, was marked by the rise of the textile industry. The changing markets of post-Black Death Europe provided considerable demand for russets, a durable, middling priced cloth that was Colchester's principal product. Russets were exported to the Netherlands, Germany, Gascony, and Italy in return for fish, salt, wine, and manufactured goods. Credit markets developed, and textile production—increasingly organized in a capitalistic fashion—grew enough to attract immigrants from the countryside and abroad. This made Colchester unique, for, when most towns were losing people, steady immigration doubled its population. Moreover, wealth made the burgesses self-confident and civic minded. They increased their political power, electing town councilors and financial officers, and made municipal improvements, such as rebuilding the Moothall and the city walls.

But expansion contained the seeds of decline. Colchester was less important than county towns, such as Ipswich and Norwich, and the capital, London. Foreigners carried most of Colchester's trade, and Colchester's prosperity did not trickle down to the countryside. Only farmers in a ten-mile radius benefited from its food markets. Furthermore, the very emigration that allowed Colchester to grow exacerbated rural depopulation. These problems continued into the third epoch, 1415 to 1525. Textile production collapsed, as luxury cloths replaced russets. As industry faded, immigration slackened and population fell; indeed, many of Colchester's young men left for London. Credit markets tightened, and ownership of the remaining industries was concentrated among a few entrepreneurs. These entrepreneurs accrued political power, generally at the expense of the commons—those burgesses who were neither bailiffs nor members of the town council. But public life was time consuming and expensive. A rich man could live better in the countryside. As textile profits dwindled, many entrepreneurs transferred their investments into land and left the city. Those who remained tried to slow

the decline by imposing market restrictions. Guild by-laws and regulations were enacted. This led to stagnation, contraction, and social tensions. By 1525 Colchester was decaying.

Britnell is not always convincing. He is hard pressed to explain why the rich kept getting richer after 1450. Moreover, others were doing well too, since the demand for beer and beef was consistently high in the fifteenth century. Change, not decay, is a better description for his last epoch. The author does not adequately explain the reasons for the rise of the textile industry; this undermines his premise. He treats textiles to the exclusion of other industries. And the book is somewhat old-fashioned. Despite the author's disclaimers, readers want to know about religion and the cultural and social life of the town. Neither economic nor institutional history can be studied in isolation.

But this is a good book. Britnell has done extensive research and knows his material. He makes excellent use of his evidence, especially documents in the Public Record Office that most local historians ignore. He provides fascinating details, particularly when he discusses the mechanics of cloth production. His arguments are cogent, well written, and never dogmatic—a refreshing change in a contentious field. Ultimately, Britnell makes clear—unfortunately for teachers and textbook writers—that there is no paradigm for late medieval urban history. Each town was different and must be studied on its own.

ROBERT S. GOTTFRIED  
Rutgers University

ALISON HANHAM. *The Celys and Their World: An English Merchant Family of the Fifteenth Century*. New York: Cambridge University Press. 1985. Pp. xv, 472. \$49.50.

To describe a merchant in her *Medieval People* (1924), Eileen Power drew on the Cely papers for the milieu of Thomas Betson. Only 132 letters and a few accounts had been published, but in 1975 Alison Hanham edited all 247 letters for the Early English Text Society. She now provides an account of the business affairs of the Celys from 1472 to 1489.

In effect, this volume is two books: a social history of the Cely family in the manner of George G. Coulton and a detailed analysis of the wool staple at Calais. Hanham's editorial expertise enables her to illuminate the nuance of meaning and correct the dating of various letters. She also has a wide knowledge of contemporary documents and literature that supplement the Cely letters. The result is a mine of information on subjects ranging from clothing to menus and from marriage celebrations to

funerals, as well as George Cely's interest in hawks, hounds, horses, and hunting. Her own words best indicate the nature of the book: "The aim of this work is to present this large amount of varied material in coherent form, and at the same time to make accessible as much information from it as possible" (p. x). I doubt that anyone could extract more from the letters; for most purposes this book will substitute for them or, at least, provide a guide to them. Unfortunately, the overwhelming detail and the number of quotations will discourage many readers from obtaining the "world" view. The author gives almost no help by failing to make generalizations or fit the information into other historical scholarship about the period. Providing context is made more difficult because, unlike the Paston letters, the Cely letters contain few references to political developments.

The middle chapters on the wool trade (about one-third of the book) make an important contribution to scholarship in their own right. Emphasis on the meaning of technical terms provides a primer on the subject. The merchant selling wool through the staple at Calais had to master arcane matters of weights and measures that differed from those in London, the intricacies of several coinages, and the money of account used by the staple. Hanham makes a number of minor and some major corrections to previous scholarship. The romantic picture of packhorses bearing wool to the ports along ancient trackways will now have to give way to more mundane heavily laden wagons because she shows that the weight of a bale (as correctly understood) was far too heavy for a packhorse. In her book on the wool trade Power thought that wool prices expressed in pounds sterling at Calais remained steady for long periods. Hanham, digging more deeply beyond these notional "official" prices, found significant fluctuation in real prices in terms of the actual coinage in which trade was conducted. A further complicating factor was the use of various contracts and bills providing for later payment or payment at another place where the price might be further affected. Hanham is at her best in explaining these complications, which sometimes baffled even merchants of the fifteenth century.

CHARLES R. YOUNG  
Duke University

CHARLES W. BROCKWELL, JR. *Bishop Reginald Pecock and the Lancastrian Church: Securing the Foundations of Cultural Authority*. (Texts and Studies in Religion, number 25.) Lewiston, N.Y.: Edwin Mellen, 1985. Pp. xvi, 274.

The name of Reginald Pecock—bishop of St. Asaph from 1444 to 1450 and of Chichester from 1450 to

1457, when he was deprived for alleged heresy—is one of the more noteworthy among the admittedly undistinguished roster of fifteenth-century English churchmen. Pecock himself was not a theological or a literary giant nor of exceptional political importance, but he did have a particular prominence in the religious and cultural worlds of his time. In any case, Charles W. Brockwell, Jr.'s, purpose was not to write a biography (V. H. H. Green's of 1945 is still serviceable) but, through a detailed study of the six extant works among Pecock's numerous writings, to explore three major themes, whose cumulative impact is summed up in the book's subtitle.

The first concerns ecclesiology and obedience, particularly in the face of the Lollard challenge. Pecock's stance was that of a conservative reformer, reserved toward monasticism, not much concerned with the papacy, and keen on the necessity of an educated clergy. This leads to the next theme, the "new Christian curriculum for use by both clerk and layman" (p. 58), which Brockwell finds explicit in Pecock's work. In fact this turns out to have a highly traditional and indeed scholastic cast (the *Seven Matters of Religious Knowledge*, the *Four Tables of Moral Virtue*), although it is striking that Pecock, albeit a doughty opponent of the later Lollards, approved the circulation under limited conditions of Scripture in the vernacular. Finally comes a consideration of Pecock's theological methodology, which was based on reason, Scripture, tradition, the sense of history, and prayer (the section on prayer is exiguous).

In a concluding "Assessment and Prospect" Brockwell solidifies and amplifies a suggestion he had adumbrated in his introduction: that Pecock "had a concern for the cultural authority of the church on a broader scale than has generally been acknowledged" (p. xii). But the meaning of "cultural authority" is never made quite clear, although the first chapter, "Church and Society in Lancastrian England," seems meant to explicate the idea. Herein lies one of the book's two major weaknesses: the author seems to employ an undergirding intellectual framework that is not really laid out; he should have made his theoretical structure explicit.

The other weakness is a certain naiveté both in understanding Pecock in his tradition (for example, a passage on page 66 in which he is merely paraphrasing the so-called Athanasian Creed is not recognized, and a theological distinction as old as the fifth century is thereby ascribed to him) and in the writing, especially in the rather arch references to "Doctor," "Bishop," and even "Father" Pecock. And the numerous notes are often integral enough to the text that they need to be foot- rather than endnotes.

These reservations aside, Brockwell's monograph contributes to a fuller understanding of Pecock's



work and of his place in the cultural and ecclesiastical scene of late medieval England.

RICHARD W. PFAFF  
*University of North Carolina*

G. A. LOUD. *Church and Society in the Norman Principality of Capua, 1058–1197*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1985. Pp. xv, 283. \$39.95.

In spite of Evelyn Jamison's helpful and provocative pioneering work, much less has been written by English-speaking historians about the south of Italy than about the north. In fact, to write about the south is more difficult. A broader range of interests and knowledge is required to understand the kingdom of Sicily than Florence, Pisa, or even Venice, and the work is less immediately satisfying. To some extent the old lack is now being remedied, but it is still sufficient to give an added value to G. A. Loud's book on the principality of Capua. The principality has, in fact, been especially avoided. According to Loud, "The reason for this lies in the assumption of an older generation of historians, writing in the shadow of the *risorgimento*, that the unification of southern Italy was an inevitable process . . . Capuan history seemed to them a dead end" (p. 2). Like many things that Loud points out, which seem to him local and particular, this neglect of Capua is related to a broader phenomenon, the more general neglect of European areas that did not develop into nation states.

Emphasis on the local and particular is at the heart of Loud's intention as a historian. He wants to tell his reader that what happened in the principality during the period of Gregorian reform was primarily shaped by local conditions, events, interests, and animosities. He underlines the slowness of change and the strength of continuity, when one might have expected abrupt change—for example, when Norman rulers replaced Lombard rulers. Naturally, for this kind of local history, Loud's technique is to narrate lots of events, to let as many voices as possible speak, and to use every possible kind of evidence from a time and place that left little religious evidence. Perhaps wisely, however, he uses, for the land of Sant'Angelo in Formis and San Vincenzo on the Volturno, as well as Cassino, less visual and tactile evidence, even less archaeological evidence, than might have been expected.

Although his approach is detailed and pragmatic, Loud is not without opinion. His presence and persuasions enliven the book even when his views are not entirely convincing—such as on the virtue of Pierleone, the island-theory phrase of Fernand Braudel, even the unusual pace of clerical reform in Capua. Whether or not one agrees with all of Loud's

generalizations, one can learn much about Capua and about political and social geography from this book. In it one hears voices not later to be forgotten, such as that of Alexander of Telesse. Such a book would be insufficiently daring if it did not make mistakes; one, perhaps, is the subjection of Ravello to Amalfi.

ROBERT BRENTANO  
*University of California,  
Berkeley*

PENN R. SZITTYA. *The Antifraternal Tradition in Medieval Literature*. Princeton: Princeton University Press. 1986. Pp. xvi, 316. \$40.00.

The most novel development to intrude on the medieval church was the appearance in the High Middle Ages of the mendicant orders. The church by that time was a well-ordered institution with secular and monastic clergy in full command of the work of saving souls. Since no place was left for the friars, friction was bound to develop even had all friars lived lives as inspiring as Francis and Dominic. In this scholarly volume Penn R. Szittyia discusses the views of unfriendly scribes, English for the most part, on the friars.

Most disturbed over the appearance of the mendicants were the rising universities, which the secular clergy dominated, bishops who resented the exemption popes had extended the friars, and the secular and monastic clergy, who found many people preferring to have friars preach to them, hear their confessions, and bury their dead.

The first serious confrontation between friars and university took place in Paris in 1253. The failure of the friars to honor a strike called by the university provoked the clash, although the basic cause was the progress of the friars toward gaining a major voice in the business of the institution. A leading spokesman for the university at the papal court, where the controversy was appealed, was William of St. Amour, a master of theology, and his tract, *De periculis novissimorum temporum*, became the most influential antifraternal work for the next two centuries. William likened the friars to the Pharisees of Christ's day and to the false prophets of the apostolic age. They were agents of Antichrist and harbingers of the End.

Since many believed that the world was in its Sixth, and final, Age, people who disliked the friars accepted the charge that they were indeed evil and harbingers of the End. Among these were monastic chroniclers who spoke for the regular clergy and some bishops such as Richard FitzRalph, primate of Ireland. In contrast to the eschatological approach of William of St. Amour, FitzRalph's attack was



ecclesiological. The church that Christ and time had ordained left no room for friars.

John Wyclif, "after FitzRalph . . . the most prolific antifraternal writer of medieval England" (p. 153), initially had only kind words for the friars. They had accompanied him to St. Paul's to help defend him against the charge of heresy before a group of bishops. When he attacked transubstantiation, the mendicants broke with him. He in turn repudiated them and thereafter never ceased denouncing them as the evil incarnate that had loosed Satan the world.

Lastly, the author introduces English poets, most of whom had little good to say about the mendicants. The poets did not appeal to the Bible or theology to rationalize their criticism. They were troubled by the vagrancy of the friars, their begging. Langland, who receives the lion's share of Szitty's attention, linked friars with other social groups whose presence symbolized the perversion of a rightly ordered life, namely, wanderers, minstrels, and beggars.

The author offers appendixes and indexes but no bibliography, although he is careful to cite monographic and periodical literature when this bears on controversial points.

Uninformed readers will leave this book convinced the friars were a scurvy lot. They will have forgotten the caveat the author dropped in his preface, that the irritants that spawned this antifraternal literature "helped to create and preserve many charges against the fraternal orders that were distorted, outdated, or false" (p. ix).

JOSEPH DAHMUS

Pennsylvania State University

ERNST BREISACH, editor. *Classical Rhetoric and Medieval Historiography*. (Studies in Medieval Culture, number 19.) Kalamazoo, Mich.: Medieval Institute, Western Michigan University. 1985. Pp. 237.

The four essays in this volume grew out of a series of sessions devoted to the influence of classical rhetoric on medieval and Renaissance historiography organized by members of the Medieval Institute at Kalamazoo beginning in 1978. As Ernst Breisach explains, this effort was stimulated in part by the recent revival of scholarly interest in rhetoric, itself linked to a growing concern in modern linguistic and communication theory with the relationship between language and the representation of reality, in part by the relative neglect of historical writing as a source for the examination of that relationship. An underlying premise of the sessions was that ancient rhetoricians, notably Cicero and Quintilian, had bequeathed to the Middle Ages and Renaissance a body of linguistic theory and rhetorical practice governing modes of historical discourse

and the construction of historical narratives. It was equally assumed that medieval and Renaissance historians, lacking alternative conceptual models for the practice of their art, readily availed themselves of this imposing legacy.

Not all the essays published here confirm the original premise. The problem is not that extensive use of classical rhetorical figures, reliance on traditional devices such as set speeches and character portraits, or even direct borrowings from classical rhetoric are difficult to demonstrate. Rather, as Nancy Partner lucidly explains in her essay entitled "The New Cornificius: Medieval History and the Artifice of Words," "the straightforward approach of applying the categories and terms of traditional rhetoric to medieval historical writings looks superficially promising, but runs quickly into blank walls" (p. 42). Despite the obvious presence of classically inspired rhetorical tropes in medieval histories, not nearly so obvious is how rhetorical principles shaped medieval historians' ways of thinking about their material and framing their narratives. With a few well-known exceptions, most medieval historiography simply does not read like its classical counterpart but as a genre falls, as Partner wittily observes, "somewhere between epic and laundry lists" (p. 9). Whereas classical rhetorical historiography sought to order and render intelligible the experience of human life, medieval historiography sought meaning in the divine and spiritual forces that lay outside the flux of secular events, the interpretation of which was, in any case, necessarily provisional until vouchsafed by God's full revelation of his purposes for humanity. Under these circumstances, the emergence of a genuinely rhetorical history was hardly likely.

To their credit, the essays by Roger Ray on rhetorical skepticism and verisimilar narrative in John of Salisbury's *Historia Pontificalis* and John Ward on principles of rhetorical historiography in the twelfth century probably make the best case possible for the pervasive influence of rhetorical concepts in the work of a select group of historians, among whom, not surprisingly, John of Salisbury figures prominently. Whether that case is ultimately persuasive is a matter for the individual reader to determine. I do not entirely agree with Ward's contention that the rhetorical *exordia* typically found in the prologues to twelfth-century chronicles are accurate guides to contemporary historical ideas, as opposed simply to a means of invoking a distinguished tradition for the exercise of a literary art that had few institutional supports in the Middle Ages. Nor will all readers concur, I suspect, with Ray's statement that "a significant part of John of Salisbury's *Historia Pontificalis* was written for laughs" (p. 88). The final essay by Donald Wilcox on the sense of time in Western historical narratives

from Eusebius to Machiavelli serves to underline once again the profound differences between medieval and Renaissance historical modalities; Renaissance historical thought was much more deeply indebted to classical rhetoric and, like its ancient predecessor, committed to investigating the meaning of events as the product of changing human activity and personality rather than of transcendent, abstract forces.

The editor and authors are to be congratulated for providing a coherent and stimulating investigation of an important topic in the study of medieval historiography, and I hope that my skepticism about the validity of the book's original premise will not deter readers from appreciating their informed and sophisticated approach to the problem.

GABRIELLE M. SPIEGEL  
University of Maryland

NORMAN GOLB. *Les juifs de Rouen au moyen âge: Portrait d'une culture oubliée*. Publications de l'Université de Rouen, number 66. Rouen: The University. 1985. Pp. xxix, 475. 380 fr.

Although economic, intellectual, and literary histories of medieval European Jewish communities exist, profiles of a total civilization, comprising archaeological, archival, and manuscript evidence respecting virtually every aspect of life and culture, are rare. Norman Golb's monumental book, based on his earlier publication in Hebrew, *History of the Jews in the City of Rouen in the Middle Ages* (1976), is such a work. The volume surveys the history and topography of Rouen from the eleventh century through the expulsions of the fourteenth century, covering, *inter alia*, Abraham Ibn Ezra's sojourn in France and England, the Tosaphists, and other scholars associated by the author with the city. Several relevant documents are translated from Hebrew or Latin into French and are richly annotated.

The tone of certainty that reverberates throughout the discussion (even when it might have been preferable to write "probably" or in statements concerning controversial subjects) might lead some readers to suspect that material has been stretched to fit the Procrustean bed of the master structure. Can Golb definitely establish that the *Torah Statutes* are Northern French (and therefore from Rouen)? Is the Cambridge Job commentary published by William Aldis Wright without a doubt the work of Berechiah ha-Naqdan? Must the structure unearthed during recent archaeological excavations have been a study-house and not a synagogue? Is the issue pivotal, since study-houses have served as prayer-houses and vice versa in traditional Jewish communities until the present? Above all is the

question of Golb's use of what he calls analytic paleography to identify several toponyms that recur in Hebrew manuscripts—DRWM, RDWS, RWM, and so on—as RDWM, that is, Rodom or Rouen. By such reconstructions many central figures in medieval French Judaism are said to have been born or at least to have resided in Rouen, making the city a major center indeed of medieval Jewish civilization. And yet, if Rouen was central, why was it, as the subtitle states, forgotten? One wishes that RDWM occurred more often as such and not as just a scribal corruption.

Unlike other, less-than-responsible books on other areas of France, which are more creative than methodologically sound, Golb's book is one of considerable substance. Many of his hypotheses, indeed perhaps virtually all of them, may eventually be proved correct. Readers would have greater confidence if these hypotheses were presented with greater caution. For example, while reading of the ascription of Rouen provenance to the "Great Mahzor," I wished that there were a colophon that presented certain evidence of this. Then, indeed, the author expresses the same wish and confesses the hypothesis to be precisely that without firmer evidence (pp. 358–59). Would that this approach were more common.

Nonetheless, the material brought together in this volume paints a rich picture of a sampling of Jewish civilization in Northern Europe. Imaginatively presented and carefully analyzed, this book will serve as a window through which to view the richness of French Jewish life.

FRANK TALMAGE  
University of Toronto

EDITH ENNEN. *Frauen im Mittelalter*. 2d ed. Munich: C. H. Beck. 1985. Pp. 300. DM 39.50.

Surveying women's fortunes in medieval society, Edith Ennen cuts a long path through time, from the sixth to the sixteenth century. In other respects, however, her perspectives are significantly restricted. *Frauen im Mittelalter* is a misleading title for a book concerned above all with German women. After broader views of women's status among early Germanic peoples on the Continent and in Frankish society, Ennen focuses essentially on German lands and only occasionally glances across the Rhine, the Alps, and the North Sea, chiefly for comparative purposes or to touch on obligatory figures such as Eleanor of Aquitaine or exemplary cities such as Paris and Florence.

A well-known scholar in medieval German economic and social history, Ennen looks at women in the Middle Ages most closely through the lenses of her special interests. Traditional in approach,

largely descriptive in method, meticulous in detail, her survey is a mine of information on certain aspects of medieval women's lives and an extensive review of German scholarship in the areas of her concern. The first of these, the early Middle Ages (500–1050), is dominated by a procession of queens and regents, accompanied by lay and religious noblewomen, with women of the rural masses more dimly glimpsed. Although these women are not firmly placed in the formative contexts of familial relationships, households, and communities, their roles and activities are described and sometimes illustrated by brief biographical sketches of outstanding individuals.

Two major themes emerge when Ennen reaches the twelfth century and the urban scene that is the heart of her book. One of them, women's often enthusiastic participation in contemporary religious movements, receives a succinct treatment inspired by Herbert Grundmann's "Frauenbewegung," although it does not pursue his and more recent insights into the complex significance of women's religious strivings as an active response to the accelerated institutionalizing of church and society. Much fuller attention is given to a related theme: the expansion of urban life and institutions in later medieval centuries. Urbanization is seen as almost entirely beneficial to women, significantly extending their legal rights, especially in marriage and inheritance, and enlarging their economic opportunities. Demonstrating this view are many pages devoted to a tour of late medieval German cities, with emphasis on diversities of evidence, and, most strongly, on Cologne—the European city probably best disposed to the productive economic roles of women. How and why women achieved this prominence in Cologne and elsewhere and how their work was related to household production, family structures, and economic systems are pressing questions not adequately considered within the thematic framework provided here.

In its substance and its concentration on topics dear to German historiography, Ennen's survey seems primarily intended for her compatriots. Its value for American students of women's history will depend on their response not only to what is offered but also to what is missing in a book hardly touched by the concerns and works of American and English scholarship in this field during recent decades. We miss more than convincing interpretive structures and fresh insights in these pages in which no women speak directly or through the kind of analysis that would let their experience speak for them. Emphasizing important facets of this experience, this useful work also underscores the need for more penetrating examination of the larger problems it poses.

MARY MARTIN McLAUGHLIN  
Millbrook, New York

## MODERN EUROPE

ERNST WALTER ZEEDEEN. *Konfessionsbildung: Studien zur Reformation, Gegenreformation und katholischen Reform*. (Tübinger Beiträge zur Geschichtsforschung, Spätmittelalter und frühe Neuzeit, number 15.) Stuttgart: Klett-Cotta. 1985. Pp. 391.

This is a collection of fourteen articles published between 1950 and 1983 by Ernst Walter Zeeden. When one notes that no fewer than nine appeared before 1960, it is striking that he was working so early on many topics and approaches in Reformation history that only later became fashionable. Among them are concerns with the later sixteenth and the seventeenth centuries; comparisons between the different confessions; interrelationships between the confessions and politics, society, and mentality; the connections between the later medieval church, Catholic reform, and Counter Reformation; the local and the practical rather than the abstract and theological; a search for common patterns of development; and, above all, the ways of promoting confessional identity (*Konfessionsbildung*) and their limitations. On all these matters Zeeden offers fruitful insights and a multitude of marvelous, colorful, and significant examples drawn from his extensive reading in both the sources and the literature.

Yet Zeeden does not seem to enjoy the reputation he deserves. This has prompted me to wonder why some historians are esteemed more highly than they ought to be, whereas others (such as Zeeden) suffer from some degree of unjust neglect. A few reasons come to mind that illuminate both Zeeden's work and the nature of the historical profession, at least in the last few decades. First, Zeeden is personally a modest man, a trait uncommon among the famous. Second, as a scholar he is particularly balanced, equally as ready to cite counterexamples as to eschew sensationalistic theses that might win him instant fame. In his attempts to discern underlying patterns he never yields to the temptation to construct oversimplified models. The widespread preference these days for what is interesting rather than—and even at the expense of—what is right is not to the taste of this admirably careful and subtle scholar. Third, Zeeden has chosen to work on nonmainstream subjects—the later Reformation, Calvinism (five of the fourteen papers treat this subject, which has never been of more than peripheral interest to German historians), and, worst of all, Catholicism and the Counter Reformation. Zeeden indirectly illumines the old, abiding prejudice on this last issue when he dryly remarks that the term habitually used by historians to label intolerance and the use of force in religious matters is "Counter Reformation," although the various Protestant Ref-

ormations were introduced and maintained by precisely the same methods (p. 63). This bias is true of scholarship in the Anglophone world, but even more so in the German, where confessional bitterness and discrimination survive today to an astonishing degree. As a Catholic working carefully on the Catholic Reformation and other eccentric topics, Zeeden has paid the price for his choices, his meticulousness, and his modesty. This is a pity, for he has contributed much to our understanding, especially on the subject dearest to his heart, *Konfessionsbildung*. To anyone interested in early modern history he has much to say.

LAWRENCE G. DUGGAN  
University of Delaware

SHIMON MARKISH. *Erasmus and the Jews*. Translated by ANTHONY OLCOTT. Afterword by ARTHUR A. COHEN. Chicago: University of Chicago Press. 1986. Pp. viii, 203. \$25.00.

Because of the enormity of the Holocaust, scholars seeking the historical roots of anti-Semitism have demonstrated increasing interest in the role played by Christian thought in the creation of an anti-Semitic social and cultural tradition. The virulently anti-Semitic writings of Martin Luther, John Eck, and Ulrich Zassius, Martin Bucer's plan for the Jews, the Reuchlin controversy, and the Spanish Inquisition have made the sixteenth century a particularly fertile ground of investigation. Yet many studies of significant personalities are defensive exercises in which authors try to prove that their subjects were not really anti-Semites, although their words, writings, and actions would indicate otherwise. Luther, in particular, has been treated in this fashion. Shimon Markish, the translator of Erasmus into Russian and an established Erasmus scholar, presents us with a new treatment of Erasmus's sentiments regarding Jews. Unfortunately, this is one more effort to demonstrate that an important sixteenth-century personality was, despite almost everything that he ever wrote, not an anti-Semite. First written in 1974 and translated into French in 1979, this work appears in English for the first time.

The author notes that he wishes to counter Guido Kisch's work of 1969, in which that eminent legal historian demonstrated the existence of powerful anti-Semitic attitudes in Erasmus's voluminous private correspondence. Markish analyzes Erasmus's religious writings and comes to the conclusion that the famous humanist hated not Jews but Judaism. But then, a survey of theological works would, quite logically, indicate opposition to Judaism. And yet, even considering Markish's own presentation, his conclusions are difficult to justify. Rather than permitting Erasmus's words to speak for themselves,

the author presents a complex exegesis to explain why they do not mean what they say. Never before did the eloquent Erasmus require the services of an interpreter to make himself clear! Markish concedes that Erasmus considered the worst human failings and vices examples of "Jewishness"; that Judaism was the main factor in a corrupted Christianity; and that Jews were in a great conspiracy to subvert Christianity, were the prime cause of most problems, and were racially incapable of conversion. Indeed, Markish notes that Erasmus even condemned Christ for demonstrating pride in his Jewish ancestry but steadfastly maintains the view that this eloquent Christian espoused only anti-Judaism. Markish also notes that Erasmus was not as bad as Luther, who irrationally blamed Jews for Moravian Christian-Sabbatarianism and Christian Anabaptist "Judaizing"—both of which were Christian phenomena and did not involve Jews. Markish never explains why anti-Semitism expressed in a Christian religious idiom, that is, the idea that God hates Jews, is not anti-Semitic. Arthur Cohen's intelligent ten-page afterword easily demolishes Markish's assumption that only pogroms or actual massacres constituted anti-Semitism and that Erasmus was not an anti-Semite because he did not personally "put a gun to anyone's head," to use Cohen's phrase. Indeed, Cohen, quite charitably, devotes much space to rationalizing Markish's peculiar logic, but Markish's views are so easily deflated by Cohen that one must wonder why the University of Chicago Press undertook this publication.

Early in the volume Markish complains that his views have been neglected by the Jewish scholarly world. I do not find this as difficult to comprehend as how Markish, and Cohen too, were able to publish a book that reveals so little familiarity with recent literature on this subject. On balance, this volume has little to recommend it.

JEROME FRIEDMAN  
Kent State University

ANDREJS PLAKANS. *Kinship in the Past: An Anthropology of European Family Life, 1500–1900*. New York: Basil Blackwell. 1984. Pp. ix, 276. \$34.95.

Andrejs Plakans points out that until recently historians were not very much concerned with the details of the social life of the vast majority of the people they studied. The study of population history, one of the early mapping exercises of this uncharted territory, began only in the late 1950s, for example. Accompanied by guidebooks by Michel Fleury and Louis Henry or by Anthony Wrigley, early explorers in the field over the next three decades exploited local archives and discovered the usefulness of the most unpromising of sources: parish registers, mus-



ter rolls, household lists, and so on. Now we have a successor handbook of equal status—dealing with many of the same sources and adopting family reconstitution as a fundamental tool—which promises to guide historians during the coming two or three decades in the next major effort to delineate uncharted social formations of the European past. If the exploration of the structures and dynamics of historical demography was exacting and tedious, however, the foray into the complicated problem of kinship promises to be an even more difficult challenge for the next generation of social historians.

People who set the agenda for pioneering work are often vague about what might be found. After giving detailed instructions about how to extract the set of “dyadic” relations from household lists, Plakans admits that we may never know the social significance of the set nor even what boundaries within or around it were relevant. He warns us that we must be deeply skeptical about the terminology of record keepers and that little information on the surface of documents will be useful for analysis of kinship. And, furthermore, what people say about their own activity is as much a mystery to be explored as the activity itself. Most of the links that historians can laboriously put together from the records will point to “domains” of activity or behavior that will probably remain closed forever to the historian. And, if one of the great desiderata of the analysis of kinship is the study of kin-groups, few of them have had their “existence demonstrated behaviorally” even by anthropologists, and Europe may never have had such groups. Plakans says rather enigmatically: “The analyst must bear in mind that the process of identifying kinship groups, wherever it is attempted, is in the final analysis a thrusting upon a variegated reality of the investigator’s own constructions as to what the social reality was among the persons being examined. This commonplace notion does not make any less real the kinship grouping and its significance in communities where the reality is very close to the construction created to explain it” (p. 193).

Plakans is part of a growing community of scholars seeking to combine anthropology and history. He has read deeply and effectively in the anthropological literature on kinship and has done important historical work on archival material from the Baltic province of Kurland. In essence, his book is an attempt to devise methods for carrying out the program of British social anthropologists, particularly Mever Fortes, for the European past. He adopts Fortes’s position that kinship comprises a separate “domain” with its own internal coherence and rules. Yet its structure is not “immediately visible in concrete reality,” whether that be the diverse data from the field worker’s notebooks or the historian’s counterpart, reconstructed configu-

rations based on records. Structures can only be patiently established by empirical tests of hypotheses, systematic comparison, and analysis of multiple inventories of personnel found in similar, related, or analogous contexts, for all of which Plakans gives numerous and useful examples. But Plakans warns that to test even the simplest hypothesis “requires the bulk of research work to concentrate as much on the preparations of evidence by means of which a test can be conducted . . . as on the test itself” (p. 122).

DAVID WARREN SABEAN  
University of California,  
Los Angeles

ALAIN WIJFFELS. *Qui millies allegatur: Les allégations du droit savant dans les dossiers du Grand Conseil de Malines (causes septentrionales, ca. 1460–1580)*. Volume 1, *Texte*; volume 2, *Annexes—notes—registres*. (Rechtshistorische Studien, number 11.) Leiden: E. J. Brill or Universitaires Pers Leiden. 1985. Pp. xlii, 455; 464–1144. 220 f.

The first volume of this huge and highly technical monograph begins abruptly with a weighty discussion of the major archival sources from the records of the Grand Conseil de Malines and the Cour de Holland, superior courts of the Netherlands. Alain Wijffels introduces in turn recent scholarship on the court, the history of the institution, inventories of its archives, the citations or “allegations” of jurisprudential authorities in the consultations (*consilia*) and other writings of the advocates or practicing lawyers in the court from 1460 to 1580, the reception of Roman law in the Netherlands, and aspects of the advocates’ legal argumentation. The extensive bibliography and numerous tables, graphs, and charts in these first one hundred pages are impressive but impede satisfactory introduction for the nonspecialist.

The nearly two hundred pages that follow are organized according to the major parts of the *Corpus Iuris Civilis* (C. I. Civ.), *Corpus Iuris Canonici* (C. I. Can.), and select monographic works cited by the advocates. With elaborate charts and diagrams that further interrupt the flow of the text, Wijffels provides an exhaustive statistical survey of the advocates’ references to early and more recent legal authorities whom they found helpful in developing their court cases. His remarkable feat provides a mass of (incompletely digested) material for other legal historians.

In the galaxy of writers cited by the Netherlands lawyers, the late medieval jurist Bartolus was still the towering traditional authority on technical points of law. Baldus, his successor and a more “philosophical” jurist, was important but far less

cited. Characteristically, Wijffels supplies good examples of how Netherlandish lawyers used Baldus in their allegations (pp. 219–22). In view of the strong tradition of the *mos italicus* among practicing lawyers in sixteenth-century courts, the preponderance of references to Bartolistic writers—medieval and Renaissance—is expected and has been suggested by other scholars for Parisian and other courts, but no one has heretofore mapped out the problem as well as Wijffels. The newer, more theoretical humanistic school of the *mos gallicus* lacked, and perhaps never did acquire, a comparable weight of authority for legal practitioners.

The remainder of volume I catalogues the dozens of Netherlandish lawyers used throughout the study, giving statistics on and discussions of their works, allegations, background, and so on. Typically, the author hesitates to draw wider conclusions based on the relatively narrow body of information he has compiled. This caution has its merits but is too close-to-the-vest. Here, as elsewhere, the author fails to develop the wider significance of his valuable study.

The even larger second volume gives a series of stupendous appendixes that inventory, first, the myriad citations of individual texts in the *C. I. Civ.*, *C. I. Can.*, and other legal texts. The first appendix proceeds text by text through the entire *Institutes*, *Digest*, and *Code* in the *C. I. Civ.*, indicating which commentaries on each text were cited by the Netherlandish lawyers in their allegations. Readers will make their own use of the book's inventories, only partly exploited by the author. For *De iustitia* (D. I. 1), for example, the lawyers cited mostly Bartolus and to a lesser extent Baldus. A mere two citations of *De origine iuris* (D. I. 2) are listed. This reflects medieval jurists' conspicuous lack of interest in this "title" or chapter, in contrast to the special attention given it by humanistic jurists because of its unique treatment of the historical origins of law. Predictably, because of their emphasis on medieval jurisprudence, the Netherlandish lawyers tended to focus in their citations of commentaries on *De legibus* (D. I. 3) on the celebrated text "De quibus" (D. I. 3. 32) and surrounding texts, which dealt with custom at the expense of legislation; such a focus is also found among the medieval commentators on D. I. 3. This and other appendixes, however impressive and technically useful, are so inadequately introduced and explained that only the most ardent specialist will readily fathom and interpret them.

Appendix 2 inventories the allegations of the Netherlandish lawyers in a different way—alphabetically, according to the commentators whom they cited. The entry for Accursius includes only six items, yet thirteen pages are devoted to the *Glossa*, nine pages to Baldus, and fifteen to Bartolus. Nine items are cited under the name of the Renaissance

jurist Andrea Alciatus, one under Francis Curtius, Jr., one under Joachimus Hopperus, four under Lucas de Penna, eleven under Ulrich Zasius—to mention the figures that have been most relevant to my own studies.

Appendix 3 covers the citations of "monographic" literature (*consilia*, tracts, and so on), as opposed to citations of commentaries on the *C. I. Civ.* and *C. I. Can.* covered in the previous appendix. Again the author arranges them alphabetically, by name of the authority cited by the Netherlandish lawyer. Once again, medieval authorities overwhelmingly prevail, but Wijffels includes Renaissance authorities such as Alciatus, Louis Le Caron, Barthélemy de Chaseneuz, Nicolaus Everardus, André Tiraqueau, and Zasius. The volume concludes with a long section of notes to the main text. There is no index.

Like many recent European legal scholars, Wijffels has devoted his energies to technical analyses and writes for an audience that cares little about the inelegant literary style. His research into citations and the statistics he provides about them takes scholars of medieval and Renaissance law into new depths, with promising possibilities for computer analysis. But the terrain needs to be better illuminated by broader interpretation of the results.

A. LONDON FELL  
New York University

GIL GRAFF. *Separation of Church and State: Dina de-Malkhuta Dina in Jewish Law, 1750–1848*. (Judaic Studies Series.) Tuscaloosa: University of Alabama Press. 1985. Pp. ix, 223. \$29.50.

Within the rapidly expanding field of modern European Jewish history, Gil Graff's work finds its niche by analyzing the attitudes of Jewish groups toward the sovereign governments of the countries in which they lived. In addition, although the well-studied France of the revolutionary period stands at the center of his study, Graff skillfully crosses over political borders and his work reflects the continuity of concerns of Jewish communities in the Western world. The book focuses on the traditional concept, *Dina de-Malkhuta Dina*, which, since Talmudic times, has acknowledged the legal authority of the secular state in a distinct number of spheres. Although for many centuries this concept applied primarily to powers of taxation and confiscation, in modern times Western governments widened their areas of interest and involvement in Jewish life and thus forced traditional leaders to reevaluate their perception of the legitimate limits of state sovereignty.

Joseph II's Edict of Toleration (1782) viewed education as a matter of state concern and control. Over the next decades military duty and family law were focal points for tension between traditional



Jewish law and expanding governmental domains. Military service presented conflicts with both Sabbath observance and the dietary laws, whereas family law involving marriage and divorce represented a particularly sensitive area of rabbinic authority. Napoleon both forcefully and dramatically brought home the message of governmental demands when he convened a meeting of French Jewish lay and rabbinic leaders. At this Assembly of Notables and the subsequent Sanhedrin meeting, Napoleon indicated through a list of twelve questions his expectation that the Jews recognize the primacy of French law. Even more, he demanded loyalty to the French nation and fraternal attachment to the French people.

The response of Western European Jewish leaders to these issues is the subject of Graff's book. Although he cites only a limited sample of rabbinic views, he demonstrates the ways in which traditional leaders accommodated many of the enactments that encroached on Jewish legal requirements. At home with juridical argumentation, Graff lucidly explains the arguments and reasoning behind rabbinic responses. Although outside observers complimented the adeptness of such leaders as David Sinzheim, Graff demonstrates that these traditional leaders acted in sincerity as they tried to protect Jewish law and values while seeking paths of accommodation in response to the improved status of the Jews in civil society.

When discussing the attitudes of the rabbinical leadership of the reform movement in Germany, Graff emphasizes the radical changes that took place in interpreting *Dina de-Malkhuta* over the century starting in 1750, as external factors represented by the state replaced rabbinic tradition as the primary force determining the legitimate parameters of *Dina de-Malkhuta*. In Graff's words, "The state, not Jewish law, was [now] the source of legal authority. In all matters arrogated by the state to its own jurisdiction, Jewish law would yield" (p. 132). But Graff's emphasis on radical representatives of the movement, such as Samuel Holdheim, may simplify the contrast yet mislead the reader concerning the more moderate mainstream of German reformers. After all, the rabbinic conferences at Frankfurt and Breslau backed off from radical positions concerning both the use of Hebrew language and Sabbath observance. For example, the reformer Ludwig Philippson, who had taken the initiative in 1844 to convene these rabbinical conferences, demonstrated his notion of Judaism's *sine qua non* when he opposed the abolition of ritual circumcision and espoused the right of Jews to protect their religion by maintaining Hebrew as the language of worship.

Ultimately, most reform leaders and certainly its rabbinate were hardly prepared to accept the disintegration of Judaism and its institutions, but, of

course, Graff is correct in asserting that social and state pressures increased greatly over the century in question and that Jewish leaders throughout the Western world found diverse ways to respond accordingly.

ROBERT LIBERLES  
Ben Gurion University

MICHEL MORINEAU. *Incroyables gazettes et fabuleux métaux: Les retours des trésors américains d'après les gazettes hollandaises (XVI<sup>e</sup>–XVIII<sup>e</sup> siècles)*. (Studies in Modern Capitalism/*Études sur le capitalisme moderne*.) New York: Cambridge University Press or Editions de la Maison des Sciences de l'Homme, Paris, 1985. Pp. 687. \$79.50.

The key to this book is the double entendre in the title regarding "incredible" Dutch gazettes and "fabulous" metal imports. Michel Morineau complains that scholars refuse to confront the data offered by the gazettes and continue to accept fables about American bullion that distort our understanding of European economic change. Despite his complaints, Morineau is known to economic historians, and the book is welcome primarily because it unites his work in one volume. Chapters deal with the Dutch gazettes and American treasure (1580–1660), Dutch gazettes and Brazilian gold (1699–1806), American bullion and the European conjuncture (1659–1720), and American bullion during the eighteenth century. Unchanged from previously published versions, each has an addendum with new material. Chapter 5 is a discussion of imports, stocks, the forces motivating the bullion supply, and the significance of those forces.

The findings significantly change the chronology inspired by Earl J. Hamilton, François Simiand, and Pierre Chaunu of massive bullion imports in the late sixteenth century, reduced imports in the seventeenth, and renewed heavy imports in the eighteenth. Morineau greatly reduces the importance of sixteenth-century bullion imports relative to existing stocks and shows that in the last half of the seventeenth century imports were much higher over a longer period. In the eighteenth century imports were recurrently high, with erratic fluctuations until a new expansion to unprecedented levels after 1780.

The problem, of course, is the connection between bullion and the economy. Hamilton, Simiand, and Chaunu created an orthodoxy that linked bullion imports to the money supply and thus to the level of prices. A vast amount of history has been written using this orthodoxy to explain the crisis of the seventeenth century, the fortunes of various governments, and the geographical and chronological shifts of European commerce.

Morineau uncouples bullion from money supply and prices, except as a secondary factor. The rates of importation and retention of bullion simply do not explain price levels. In the sixteenth century, for example, the purchasing power of silver fell twice as fast as new bullion supplies can possibly explain. Money and bullion were not coterminous, and governments and banks created monetary instruments as needed. Bullion made a difference in that governments with access to bullion possessed a form of foreign exchange that allowed them to command resources at a distance. Governments with bullion-rich economies were also under less pressure to create money via debasement to capture domestic resources.

Money supply and prices thus become functions of the contexts of various European states, and Morineau (correctly, I think) presents the Continental monetary system as a series of autonomous regional units with limited interactions. Thus, general models based on close links between bullion supply, money supply, and prices have little explanatory power. In fact, the role of bullion is best understood if it is treated as just another commodity with market value. It was used for ornamentation, for consumer durables (cutlery, candlesticks, church implements), for hoarding, and for monetary tokens that circulated alongside other monetary instruments. Its high market value meant that in periods of instability it was the preferred monetary medium, but those who produced bullion saw it as a commodity to be marketed, not as money in itself.

Ultimately, says Morineau, to understand the changing European economy we must stop forcing conjunctural indicators into the chronology imbedded in the model relating treasure, prices, and conjuncture. We must instead acknowledge the impact of political life, examine the growing monetarization (and market orientation) of European society, and reexamine the transition to sustained economic growth. Morineau has investigated a phenomenon long considered to be of crucial importance and finds that it was actually of secondary significance. His work is a signal to historical economists that it is dangerous to be seduced by apparently systematic and homogeneous data from the past and to economic historians of Europe that they must read more widely as historians.

DAVID RINGROSE  
University of California,  
San Diego

RANA KABBANI. *Europe's Myths of Orient*. Bloomington: Indiana University Press. 1986. Pp. ix, 166. \$19.50.

For hundreds of years Western writers, travelers, poets, and painters have depicted the East in a manner that accords more with their own fantasies than with reality. Out of their paintings and writings has emerged a series of images that continue to feed the popular view of the "other" in general, and of Islam and Middle Eastern society in particular: images of sensual harems and of cruel sultans, of women versed in the arts of erotic play and tantalizing desire, seducing men both passionate and violent. Western imaginations, starved of such possibilities within their own societies, feed on the opulence and mystery that surround these characters and the scenes in which they are depicted: naked slave girls, black guardians of the seraglio, and sword-bearing warriors fill the pages of "real" travel accounts and of fictional tales alike.

Such are the myths to be exposed by Rana Kabbani's book. She examines their development in the work of a number of "influential" writers, mainly in the nineteenth century: Antoine Galland, Edward William Lane, Charles Montagu Doughty, T. E. Lawrence, Richard Burton, and so on. Hers is not a general history of travel but an exploration of how such individualistic and idiosyncratic writers nevertheless worked within a "communal image" to create a "literary orient."

In the last decade the analysis of how Europe constructed its "other" has developed into a full and rich genre, with Edward W. Said's *Orientalism* (1978) only the best known of a large and scholarly tradition. Kabbani's book consists mostly of summaries of this literature, with occasional comments and insights. She provides a readable and clear introduction to the field that might be particularly appealing to undergraduates. She does not, however, relate much that is new, and at times the insistent repetition of yet more authors and texts in which the classic stereotypes are to be found dulls the basic interest of the theme.

Kabbani particularly stresses the theme of sexuality and imperialism: "to perceive the East as a sexual domain, and to perceive the East as a domain to be colonised, were complementary aspirations" (p. 10). Orientalist painting, in particular, lends itself to such a treatment, and Kabbani's chapter on this subject, "The Salon's Seraglio," is probably the most stimulating in the book. Again, however, those wanting fuller examinations of the subject should refer to the manifold academic literature, such as recent analyses of the harem genre by Olivier Richot and Linda Nochlin.

Kabbani firmly locates the motivation for such exotic fantasies in the home society, in particular in the deep-seated difficulties experienced by Victorian men in relating to women. The European image of woman as wife, servant, or prostitute, she claims, underlay representations of Eastern women,

which operated free of the constraints and guilts of home: nudity and sensuality could be portrayed in "lieux" removed from contemporary surroundings; the harem offered the possibility of transgression and of "multiple" possession of sensual women in a fantasy world that counterposed the mundane and more complex reality of life in nineteenth-century Europe.

Kabbani adds some insights to these well-worn themes. Whites and blacks, and men and women, she suggests, could not be depicted as equals in this literature: one was always at the mercy of the other—even when the classic colonial situation was reversed and whites or men were depicted in subject roles, the representations served to reproduce, and perhaps lend a frisson to, the underlying hierarchical framework. Similarly, Kabbani offers a psychological explanation for the frequent representation of Eastern women as desiring Western men: the real dependency of Eastern women, given the economic and political structure of imperial relations, was sublimated beneath representations of their emotional dependency on the Western hero. Men wanted to dominate by consent and preferred not to face up to the real sources of their power; this self-deception underlay the distortion in Europe's representation of the Orient.

We are not, however, given much help in deciding how the "real" situation could be represented. Toward the end of the book Kabbani provides brief pen pictures of such writers as Wilfred Blunt and Doughty, who at least challenged some of the cruder excesses of imperialism, but in the end they too were conditioned by its ideological as well as its economic force and do not provide a model of the better practice Kabbani wishes for. Her hope, nevertheless, is for a "West-East discourse liberated from the obstinacy of the colonial legacy" (p. 13), and she sees her contribution as a review and rejection of the inherited representations that prevent such a discourse. That is perhaps as good a place as any to start, but, after Said and others, the contemporary reader is likely to be looking for more complex reviews of the ways in which that colonial ideology reproduced itself and of the relationship between literary representation and social action.

BRIAN V. STREET  
*University of Sussex*

JOHN N. KING. *English Reformation Literature: The Tudor Origins of the Protestant Tradition*. Paperback edition. Princeton: Princeton University Press. 1982. Pp. xvi, 539.

The distinctive character of mid-Tudor literature is powerfully evoked by one minor writer, William Samuel, in his *Abridgement of Goddes Statutes*: "My

mind is that I wold have my contrey people able in smale some to syng the hole contents of the byble and where as in tymes past . . . mynstrells were wont to syng fained myracles . . . and Robin hode, instede thereof to sing undoutyd trutthes, canonycall scriptures and Gods doynges." John N. King argues that the writing of the 1540s and 1550s should not be dismissed, as C. S. Lewis's did, as "Drab age" literature. Instead, it possessed a vitality derived not from an aesthetic ideal but from a powerful conviction that imaginative writing could be employed in the service of a new religious truth.

The prophet of this new vision, John Bale, sought to identify the native literary tradition of Langland and Chaucer with the assault on popery and on the clerical dominion of the papistical prelates. The death of Henry VIII and the ascendancy of Somerset, as lord protector, provided the circumstances in which literary reformism could develop its own powerful identity. Somerset liberated the press from the constraints of the previous reign and offered writers a greater freedom of expression than they were again to enjoy before the 1640s. At the same time the evangelizing preachers, who had had to exercise such caution under Henry VIII, were freed to begin the great work of the conversion of England. They sought modes of communication that would reach all sections of the populace, and the result was the emergence of a distinctive English Protestant literary tradition that found its apogee in the writings of Milton. The forms of this tradition were diverse: at one extreme biblical exegesis, the liturgy, and sermons used literary tropes and powerful imagery to convey spiritual messages, at the other interludes, allegories, and political tracts drew on the newly translated scriptures. Common elements linking these diverse forms were a concern for direct modes of discourse, best exemplified by the *sermo humilis* style of Latimer's works, and a conviction that the godly prince was divinely appointed to reform the church and state. Although this outpouring produced few literary giants, it included a number of important authors, whose contribution, King convincingly argues, has been consistently undervalued by Tudor specialists. Paradigmatic figures, who merit particularly serious attention, are Robert Crowley and William Baldwin. Crowley stands as the translator of the Langland tradition into the mid-sixteenth century; Baldwin, as the most exciting of the imaginative writers.

Historians should warmly welcome this attempt to rehabilitate the literature of the mid-Tudor period, especially since King offers such rich insights into the relationship between his authors and the religious changes of these years. The publisher's claim that the survey is comprehensive for once seems justified; the book will obviously stand as an important work of reference on both major and minor

literary figures. Sometimes this comprehensiveness seems disadvantageous to the reader who is eager to pursue King's analytical insights rather than the byways of Tudor publishing, but it is no doubt a necessary aspect of the evaluation of an entire tradition. More serious questions arise about the rather cavalier dismissal of the classical humanist vision of Thomas More and his circle. The midcentury authors certainly reverted to an enthusiasm for native culture not evident among the humanists, but, as the author himself acknowledges, they remained sensitive to audience and context. Court revels continued to use classical motifs, the learned wit of university drama owed more to Rome than to native dramatic traditions, and Baldwin's writings reveal a sophistication that is dependent both on Chaucerian influence and on the humanists. The historian, however, is on firmer ground in questioning a literary colleague on his historical analysis than on his study of form and genre. In his eagerness to demonstrate the distinctive character of the Somerset years, King argues that the duke himself was actively committed to the ideal of freedom of conscience. He endeavors to avoid anachronism by insisting that this was toleration only for those who accepted the truth of the gospels, but we are nevertheless left with the old historiographic image of the "good duke," enthusiastic for liberty, rather than an insecure ruler who saw the lifting of restrictions as a means of gaining political support. Of course reformers and preachers were right to be grateful for the consequences of his actions, but, in comparison, for example, with the sustained efforts that Thomas Cromwell made for writers, Somerset's direct contribution must appear minor. Belief in the godly prince was an article in the reformers' creed and is well analyzed in this volume: historians need not value the motives of Tudor rulers quite so highly.

FELICITY HEAL  
Jesus College  
University of Oxford

G. E. AYLMER. *Rebellion or Revolution? England, 1640–1660*. (OPUS.) New York: Oxford University Press. 1986. Pp. 274. \$24.95.

G. E. Aylmer's book is evidently based on an impressive mastery of the rich body of primary sources for the Civil War and Interregnum and of the equally massive secondary scholarship. Aylmer provides a concise account of the complex political and religious developments of the period and maintains his command of the narrative even in tackling those peculiarly contorted incidents in which the period is so fertile—the king's tergiversations in 1646, for instance, Monck's maneuvers twelve years later—and which have reduced so many historians to

prolix incoherence. His analysis, although rooted in political and religious events, extends to an examination of the major social issues on which recent scholarship has focused, and his conclusions are invariably solid and judicious. The book, which incorporates a thirty-six-page table of events and an equally extensive series of suggestions for further reading, is, then, a thoroughly useful contribution. It is also, regrettably, somewhat tedious going. Aylmer writes about a period of intense conflict that witnessed the most fundamental challenges to established values and culminated in regicide—a period that has generated intense historical controversy—in a curiously bloodless way.

Part of the problem may stem from Aylmer's understanding of the editorial injunctions governing the series in which this book is published. The OPUS series is intended "for the general reader as well as for students." An endeavor to accomplish this goal is certainly suggested by Aylmer's frequent, occasionally incongruous, use of modern parallels to explicate seventeenth-century events and attitudes—is our understanding of Gerrard Winstanley really enhanced by suggesting that he in some sense anticipated Lenin's "dictum that Communism equals Soviets plus electrification" (p. 120)? It may also underlie his reluctance to permit himself anything but the most limited citation from the vibrant primary sources for his period. This was a tactical error, making the book less, not more, accessible to a general audience. His argument could have been reinforced and driven home by apposite quotation—Cromwell's bitter expostulation, "Good, by this man against whom the Lord hath witnessed!" in relation to the army's sense of Charles's perfidy and blood-guilt in late 1648, for instance. Certainly, the liveliness of the book would have been much enhanced. The concern for multiple audiences, student and general, may have created another problem for Aylmer: how to present the vociferous disputes that have marked the historiography of the English Civil War. His solution to this dilemma unfortunately enhances the flat tone of the book. He catalogues the various debates and provides thumbnail sketches of the arguments of the protagonists, but these slight synopses, though eminently even-handed, provide little sense of the passion with which the issues were discussed or, worse, of the evidential grounds or conceptual assumptions that underlay the differences. Symptomatically, the issue ostensibly raised in the book's title, an issue that has exercised historians from Clarendon through S. R. Gardiner to the paladins of modern revisionism, is directly addressed only in a short final paragraph that skates over the fundamental questions before pronouncing an eminently sensible, moderate conclusion that, *inter alia*, questions the central position that the Civil War has enjoyed in our historical



consciousness. Aylmer's general audience may too easily share this sentiment, given that his book reflects so little of the vigor of the period or the excitement that it has inspired among generations of historians.

CLIVE HOLMES  
Cornell University

JOAN THIRSK, editor. *The Agrarian History of England and Wales*. Volume 5, 1640–1750. Part 1, *Regional Farming Systems*; part 2, *Agrarian Change*. New York: Cambridge University Press. 1985. Pp. xxxi, 480; xxx, 952. \$153.90 the set.

A score of collaborators have produced a full rural, rather than merely agrarian, history and have gleaned a great deal of valuable detail that essentially confirms previous findings in agrarian history and gives new and interesting information on rural industry and building and on market gardening (truck farming).

How far the contributors have complied with the canons of historical research may be judged, first, by the extent to which they have consulted the primary sources. One clue is provided by the frequency with which "the" is rendered as "ye," "that" as "yt," or even "them" as "ym," for this betrays in all but seven of the essays an inability to recognize a thorn or a lax "th," suggesting insufficient historical training, especially among the geographers, economists, and economic historians. Although some contributors have evidently done much or most of their own research, assistants were employed, largely at public expense, to select and transcribe most of the manuscript documents used in the book as a whole. No serious historian would willingly forego the joys of discovery or entrust another person to procure evidence crucial to an argument. Moreover, this kind of division of labor invites mistakes and may account for some of the errors to be found in this work. Furthermore, some contributors have failed to inspect readily accessible primary sources and secondary studies. Edward Lisle's *Observations in Husbandry* (1757) should never have been cited indirectly and at second hand. Editor Joan Thirsk is herself not entirely blameless. She frequently fails to proceed from the calendar to the state papers themselves and, despite the pertinent remarks by Wallace Notestein and Roland G. Usher in 1916, shows no sign of having gone from the printed *Journals of the House of Commons* to the originals.

Whether the contributors possess the technical knowledge required for agrarian history may be judged by how far they understand the relevant terms of the art. Sad to say, some of the authors partly or wholly misunderstand such technical terms as "arable," "watermeadow," "partible inheritance,"

and "beneficial lease," showing a lack of familiarity with the subjects.

Shortage of time can hardly be pleaded, for the work was ten years in the making. Not all the contributors, however, took their full time. One of the chapters appears to have been written in 1979, whereas some show evidence of hasty composition or proofreading, and references to the titles of some books are slightly inaccurate.

In assigning tasks in English agricultural history to imperfectly trained individuals, no better way can be found than to allot each a region consisting of a county or group of counties. That the local records are largely in the keeping of county councils and the national ones are often arranged by counties then proves a convenient circumstance. Having studied the sources pertaining to the counties assigned to them, historians will be in a position to follow geographical discipline by rearranging their findings according to farming countries or agricultural regions. Then, if the object is a nationwide study, the portions of countries that appear in two or more areas of assignment may be joined up, thus replacing an administrative map with an agricultural one. This general method has been faithfully followed in the present volume, as, indeed, in the preceding one. Unfortunately, the criteria chosen for determining agricultural regions are unsuitable. Instead of building on the work of William Marshall and defining regions according to their general plans of management, an attempt has been made to categorize farms by their contents, resulting in regions first classified as pastoral, intermediate, or arable and then subclassified according to their products. The general result is somewhat unsatisfactory, uninformative, and confusing. (A further complication is that this volume adopts a system of subclassification different from that in the preceding one.) The drawbacks of this system are exemplified by the descriptions given to the bare, bleak uplands of the North Country and the lush, gently undulating countryside of High Suffolk. Both are classified as pastoral; the North Country with "subsistence corn with cattle rearing and sheepkeeping," and High Suffolk with "subsistence corn and cattle with other enterprises, e.g. horses, fruit, hops and substantial dairying side" (part 1, pp. xx, xxi). This classification obscures the obvious and fundamental fact that High Suffolk was largely permanent arable and the North Country mainly in temporary cultivation only. Besides, the main strength of early modern English agriculture was its combination of arable and pastoral farming. Understandably, the system of regional classification adopted has caused misgivings among a few of the more perceptive contributors, who seek escape in natural or Marshallian regions, revert to administrative regions, or warn against "a *morcellement* too extreme to be generally

useful" and "the temptation to indulge in infinite refinement and subdivision" (part 1, pp. 197, 317).

Most of the contributors of agricultural chapters lack the detailed local knowledge needed for the accurate delineation of agricultural regions, perhaps through undue reliance on probate inventories, which relate to whole ecclesiastical parishes rather than to particular locations within them. The Brede and Pett levels are unaccountably ignored. Kesgrave and Martlesham, which consisted largely of heaths and sheepwalks, are included in the dairying country of High Suffolk. In apparent ignorance of their lay-out in narrow strips stretching from the valley bottoms to the topmost downs, Chalk Country farms such as those in Amesbury and Wilsford are cut in two; the downs where the sheep fed by day are put in one region, the fields where they were folded by night and the meadows where the ewes went in spring, in another. Denbigh Castle Meadows, Myddleton Park, Brookhouse, Whitchurch, and Kilford, some of the richest and deepest lands in Britain, appear as poor pastoral uplands. Dunstable and its downs are mapped as if they were below instead of above Chalk Hill.

Economic matters are not handled as well as they might have been. Peter Bowden's chapter on agricultural prices, wages, profits, and rents is based in great part on the appended raw statistics supplied by Giles Harrison, who freely acknowledges the multitude of various, inconstant, and often unfathomable local weights and measures. In processing these statistics, however, Bowden found he could not usually avoid treating the weights and measures as though they bore their normal present-day values. He then applied the statistics to agricultural products of largely unknown quality in transactions whose terms cannot be exactly discovered. Wool prices, for example, relate to often unknown quantities of several fundamentally different kinds of wool that were sold in different qualities for various terms of credit and paid for in irregular installments. Nobody bought wool as such, only particular consignments of specified types of wool on specially arranged terms. The further one generalizes from individual transactions to the totality of dealings and prices, the less the meaning and the more the pitfalls. It follows that all Bowden's processed statistics are of doubtful accuracy, and, to be fair, he himself treats them very gingerly. As for the cause of the main price movements, he argued in volume 4 that the growth of population stimulated the rise in prices. Here he argues that the slackening of population pressure may be supposed to have caused prices to level off. All he has left to do now is to find out what the population was at various times. We are here presented with a complete, albeit tentative, demographic theory of business cycles, with-

out even a breath about Joseph Schumpeter's alternative explanation.

John Chartres's chapter on the marketing of agricultural produce devotes to a complete list of fairs no fewer than a dozen closely printed pages, a few of which could well have been spared for a treatment of the nature of metropolitan markets. Thirsk, in the course of a comprehensive review of public policy, comes to conclusions on the effects of corn bounties diametrically opposed to those of one so expert in the subject as Adam Smith, whom she does not even mention.

The best contributions are Christopher Clay's excellent chapter on landownership, Malcolm Thick's on market gardening, Eric Evans's on tithes, Harrison's on southwestern England, as well as his notes on weights and measures, and the profusely illustrated history of rural building by Maurice Barley and Peter Smith.

As a whole, however, this book can safely be recommended only to those knowledgeable and discriminating enough to be able to separate the wheat from the chaff.

ERIC KERRIDGE

*University College of North Wales*

STEVEN SHAPIN and SIMON SCHAFER. *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton: Princeton University Press. 1985. Pp. xiv, 440. \$60.00.

Centering their attention on the controversy between Boyle and Hobbes over the experiments with the newly invented air pump and the question of the existence of a vacuum, Steven Shapin and Simon Schaffer have ventured beyond ordinary history of science or history of ideas to produce a novel "exercise in the sociology of scientific knowledge" (p. 15). By transcending the doctrinaire statements that abound in "the theoretical literature in the sociology of knowledge," which they admit was "a major and continuing source of inspiration to our project" (p. 15), the authors have deserted the customary arena of debate concerning historical sociology of science to produce a historical study rich in new interpretations and notable for the use of sources of a kind not hitherto fully exploited by scholars.

To do full justice to so rich a book is difficult in a short space. Among its many outstanding features is the analysis of what the authors call an "experimental polity" that developed among British scientists and their associates in the seventeenth century. The quarrel between Boyle and Hobbes is seen to have been not so much a face-to-face encounter between Cartesian plenism and vacuism (or between dogma and experimental science) as a conflict between two fundamentally different polities. "The experimental



polity" of the community associated with the Royal Society and its ideals was "said to be composed of free men, freely acting, faithfully delivering what they witnessed and sincerely believed to be the case" (p. 339). This community used its freedom responsibly and "publicly displayed its capacity for self-discipline" (p. 339). Hobbes, however, "proposed that philosophers should have masters who enforced peace among them and laid down the principles of their activity" (p. 339). From this point of view Hobbes and Boyle stood for "radically different solutions to the question of what was to count as knowledge" as well as how such knowledge was to be obtained and validated (p. 332). They even disagreed on the degree of openness of "special interest" groups such as the Royal Society. We may note, among many examples, the originality of Shapin and Schaffer's analysis of the debates concerning the public or private nature of experiments and the significance of "collective witnessing" for the validation of the new experimental philosophy.

One of the most original sections of the book is a meticulous and detailed analysis of the contemporaneous attempts to replicate Boyle's air pump and to reproduce his experiments—features commonly said to be among the primary characteristics of the new science. The authors find, however, that only a small number of the pumps Boyle designed were successfully built during the decade of the Boyle-Hobbes controversy in the 1660s; despite Boyle's meticulously detailed "reporting practices, no one was able to build a pump and make it operate without seeing the original" (p. 20).

Although the authors argue valiantly in conclusion for a close relationship between the history of science and the history of politics, this may appear less successful than the more general thesis that "solutions to the problem of knowledge are solutions to the problem of social order" (p. 332). Who could doubt that the wants of the Restoration community addressed by the experimental philosophers "spread across Restoration economic, political, religious, and cultural activity" (p. 340)? As the authors note, however, and as others have pointed out before them, the new science did not at once produce a series of major innovations in technology (as Bacon and his followers had promised), nor did it do so for another century. Hence, "the legitimacy of experimental activity and the integrity of laboratory and scientific role" tended to be assured primarily by arguments from nature concerning "the existence and attributes of the Deity" (p. 340). Publicists of the Royal Society argued that theirs was an ideal community, in which "free discourse did not"—as Shapin and Schaffer insist—"breed dispute, scandal, or civil war" and which accordingly might be considered "an idealized reflection of the Revolution settlement" (p. 341). The authors do not, however,

provide convincing evidence that the political theorists actually used the Royal Society as a primary example of such a community—even though their goal was "to establish that what the Restoration polity and experimental science had in common was a form of life" (p. 342).

The vigor and originality of this study make us regret all the more that the authors have not made more of the critics and satirists of the Royal Society. Further, the authors have not even ventured any speculations on the relations between scientific and civil polity in seventeenth-century France or in the Netherlands and Germany. In the concluding pages the authors leap through time to the problems of science and society (or the state) in our own days—expressing a point of view that may have predetermined some of the analysis of the affairs of the seventeenth century.

The authors conclude their magnificent study with the sentiment, "Knowledge, as much as the state, is the product of human actions. Hobbes was right" (p. 344). It is fitting, therefore, that an appendix contains an English translation (done expressly for this volume by Schaffer) of Hobbes's *Physical Dialogue* (1661), a polemical response to Boyle's account of his experiments and conclusions concerning the air and the void (1660).

I. BERNARD COHEN  
Harvard University

GARY STUART DE KREY. *A Fractured Society: The Politics of London in the First Age of Party, 1688–1715*. New York: Clarendon Press of Oxford University Press, 1985. Pp. xvi, 304. \$34.50.

CLAYTON ROBERTS. *Schemes and Undertakings: A Study of English Politics in the Seventeenth Century*. Columbus: Ohio State University Press, 1985. Pp. xiii, 333. \$35.00.

These two excellent studies demonstrate the advantages for historians of Stuart Britain in moving away from the overworked topics of the English revolution of the 1640s and 1650s to examine the social, economic, and political developments of the half-century after 1660. Clayton Roberts documents the development of a crucial new technique of political control. Gary Stuart De Krey's scope is wider; he examines the place of London in politics during the period of the "rage of party."

Roberts convincingly documents the emergence of the parliamentary undertaker, from the first unsuccessful experiment of Neville in 1610–13 and the abortive "Bedford plan" of 1641 to Robert Harley's sophistication of undertaking techniques in 1708–13. The revolution of 1688 played a central role in the two developments Roberts emphasizes. Before 1688 attempts at undertaking were episodic,

being linked to actual or incipient political crises—the breakdown of the Elizabethan consensus in 1610–14, the drift toward conflict in 1641, the malfunctioning of government in the decades after 1660. But after 1688 no sovereign or minister could contemplate even a short period without a parliamentary session, so that every administration had to employ the political techniques of undertaking and management. An important (and in my judgment correct) distinction is made between the undertaker, who after (but not before) 1688 had no choice but to rely on the support he could enlist from a party as well as the credit he could establish in Parliament, and the manager, who relied on patronage to marshal dependents and appeals to loyalty to mobilize independents.

Roberts deals with the emergence of new forms of high politics, located in both the royal court and Parliament. He examines the innovative techniques of manipulation and control used by those at the top or, even more ruthlessly, by politicians driven by ambition to get to the top. I have three main criticisms of his generally convincing treatment of this important subject. He could have brought out more clearly the extraordinarily open political and social position that enabled the most aggressive and adroit men in the post-1660 generation to advance themselves. Apart from Sunderland, Buckingham, and Wharton, the most successful were self-made men climbing from the status of substantial gentry or younger sons to become ministers and consequently viscounts, earls, or even dukes and, in many cases, to found enduring political dynasties. Further, he underplays the impact made in England by Scots, and he tends to neglect the rise of the monied interest, the London-based financial and commercial elite after 1688.

Concentrating on this last aspect, De Krey's study complements Roberts's thesis by examining the relationship between politics in London and their underlying trends with those of the nation generally. The comparisons he makes are of fundamental importance for the specialist and the general student of late Stuart political and social history. This is an excellent study, certainly the best we have of the internal structures of the capital and of the part it played in national affairs. De Krey's most informative achievement is to demonstrate in detail how the two parties virtually exchanged characteristics: the Whigs, popular and almost democratic in 1688–90, became a defensively minded oligarchy by 1710, whereas the city Tories, who had been installed in power by Charles II's prerogative powers in 1683, operated after 1707 as an openly populist party of constitutional reform. De Krey connects this development with the spread of a libertarian perspective among the "commons," (the artisans and middling tradesmen). Alienated by what they saw as the

apostasy of the Whig oligarchs, who concentrated on their connections with the Whig ministers of Queen Anne, the majority of voters in the outer wards of the city became the backbone of the Tory party. De Krey passes over the perhaps familiar story of the Sacheverell riots and the disturbances of 1715 too lightly, but he has a most convincing section on the effectiveness of Tory journalism in raising the political consciousness of the urban middle class in forms that lasted for a generation.

Even more valuable is his exposition of an extremely important thesis on the connections between the division of London between Whig and Tory and the new social and economic divisions largely created by the French wars, the establishment of religious toleration, and the development of the credit system based on the Bank of England. In a careful analysis of the complexities of the structure, interests, and relative wealth of the business classes, De Krey asserts that the dominant Whigs of the 1690s represented new money and a new generation. By the 1710s they were a wealthy, self-centered elite manipulating the city's constitution and were further characterized by their dissenting faith and their favor to foreign immigrants. Both these books are essential reading and have the additional merit of suggesting new topics for research and new lines of approach.

JAMES R. JONES

*University of East Anglia*

LEON RADZINOWICZ and ROGER HOOD. *A History of English Criminal Law and its Administration from 1750. Volume 5, The Emergence of Penal Policy*. London: Stevens and Sons; distributed by Carswell, Agincourt, Ontario. 1986. Pp. xv, 1101. \$126.50.

The history of crime, police, and punishment was once written as straight whig history, for which no apologies were offered. The squalor, brutality, and chaos of the old penal system of the eighteenth century and before gave way to the onslaught of humane reformers who were more interested in rehabilitating prisoners than brutalizing their bodies.

By the 1960s historical revisionists had mounted their assault, and the whigs had to take cover. Far from seeing the late eighteenth- and nineteenth-century developments, especially prisons, as the triumph of humanitarian reform, critics began to emphasize the coercive authority of the state and to put prisons along with factories, workhouses, mental asylums, and juvenile reformatories into what Michel Foucault called a "carceral archipelago," representing the exercise of state authority and the maintenance of class domination. In the past few years a counterrevisionism has called these oversim-

plified views into question and has deemphasized state authority, preferring to look at the exercise of power as complex and reciprocal, crossing class boundaries.

For almost forty years Leon Radzinowicz's study of the English criminal law after 1750 has marched along resolutely, and this fifth volume, after a gap of eighteen years, brings the study up to the early twentieth century. Radzinowicz and Roger Hood have not intended to start any fights or erect a new orthodoxy, and historians with a taste for methodological combat will have to delve into articles and monographs. The authors have set out to tell as much as possible about the twists and turns in official policy regarding Britain's law breakers.

This is administrative history at its best, specifically, a history of the evolution of public policy, and little is introduced here from outside the field of view of policy makers. Victorian social engineers were concerned with deviance in its various forms, extending out to juveniles, vagrants, drunkards, and even political offenders. The work begins with a theoretical discussion of crime as interpreted by, among others, Marxists, classicists, and positivists (who maintained that criminals were born, not made; this made little headway among British penal thinkers). Even the eugenicists had their say.

The section on political offenders is especially interesting. Political crime as a thing apart has a long history in England. Given its topical significance with regard to hunger strikers from the Irish Republican Army a few years ago, the Victorian and Edwardian approach to the classification and punishment of offenders whose crimes were politically motivated (for example, the Chartists, the Fenians, and the suffragettes) is particularly significant. Unlike European legal codes, the English common law did not provide for a separate political category, but in practice such offenders were treated differently than other criminals, usually more leniently, although not so with Chartists such as Ernest Jones.

With its distinct whig flavor the book details a more humanitarian approach throughout the penal system and a softening of penalties, many of which had been devised as alternatives to capital punishment (which itself was used less and less): the abolition of transportation overseas, shorter sentences, improved prison conditions, the development of juvenile institutions, and probation. Flogging, however, had its admirers, and it remained in use. Even early in the twentieth century the whipping of vagrants enjoyed a brief resurgence in some areas of England. All of this happened against a backdrop of what Victorians recognized as a reduction of crime, which in turn encouraged them to look to social, economic, and moral reforms to improve society, rather than to rely on penal repression.

Radzinowicz and Hood have well organized their massive research and have provided an interesting quarry for further study. The bibliography alone takes up 258 pages; it is extremely useful and includes a number of works in other languages. With its sober black cover and 1101 pages, the book's dimension would not on sight suggest casual reading, but, like the previous volumes, it is fascinating and well written and of course uniquely comprehensive.

PHILLIP THURMOND SMITH  
*Saint Joseph's University*

JAMES KING. *William Cowper: A Biography*. Durham, N.C.: Duke University Press. 1986. Pp. xiii, 340.

Although he spent most of his life in seclusion away from the literary life of London, William Cowper (1731–1800) became one of the most famous poets of his era. James King wanted to write a biography that was more factually accurate than the many others already available. It is based on printed primary sources and uses many manuscripts for the first time. The book undoubtedly has value for its intended audience of literature specialists, but few historians will find it very useful—not even those interested in crossing disciplinary lines.

Some biographies deal with a man and his times. This one focuses almost exclusively on Cowper himself—his feelings, emotions, internal struggles, and sufferings. King argues that the death of Cowper's mother when he was six years old dominated his entire life because it led to horrible feelings that "arose from his sense of separation, isolation, and desertion" (p. 5). King's venture into psychohistory is elementary and not very convincing. His thesis is little more than a statement of faith and, in any case, by explaining everything, explains nothing.

Topics are discussed only if they are related to the poet's literary life. Cowper "often espoused radical causes in his letters" (p. 162), for example, but virtually no hint of this side of his thought ever surfaces in this biography. Although the beginnings of the French revolution would certainly have interested any man who "espoused radical causes," that great upheaval is not mentioned; neither are the poet's reactions to it. King is interested only in those aspects of Cowper's life deemed relevant for understanding a man whose "entire waking life was devoted to literature and the pursuit of reputation" (p. 180).

Cowper's relatively quiet, sedentary life does not provide the material for an exciting biography, but nonliterary readers may find value in its depictions of the everyday life of a minor English gentleman in the late eighteenth century. Many historians and

other academics could benefit from emulating King's graceful style of writing.

WILLIAM ROOSEN  
Northern Arizona University

OLIVIA SMITH. *The Politics of Language, 1791–1819*. New York: Clarendon Press of Oxford University Press. 1984. Pp. xiii, 269. \$27.95.

In this book Olivia Smith argues that the political and intellectual ferment created by the French revolution in England engendered not only a transformation in the language of politics but an equally far-reaching contestation in the politics of language. Starting from the well-known debate between Edmund Burke and Thomas Paine, Smith traces the emergence of a "vernacular written language" capable of conveying complex moral and political arguments to the new democratic audience created between 1790 and 1820. As her study demonstrates, this was far from a straightforward process. In different ways Burke's *Reflections* and Paine's *Rights of Man* broke through the canonical eighteenth-century distinction between vulgar and refined languages, a theory that, because it pertained both to language and mind, denied the possibility of an intelligent moral vernacular speaker. This predominant position, inscribed in Samuel Johnson's *Dictionary*, the most widely read grammars, and Lord Monboddo's philosophy of language, ensured that arguments over language and the suffrage were intimately connected; the question of who was fit to participate in public discourse concerned both and helps explain the political importance attached to grammar and language reform by radicals as diverse as William Hazlitt, William Cobbett, and Thomas Spence. It also elucidates more clearly than previously the common cause that briefly held together the plebeian radicals and the poetic reformers Coleridge and Wordsworth.

The elitist position on language and mind had to be broken, both in theory and practice. Paine's writings broke through the distinction in practice but proved extremely difficult to follow. Smith explores the difficulties experienced by radicals such as Daniel Eaton and John Thelwall in their attempts to find an appropriate mode in which to address their new and scarcely known constituency, Burke's "swinish multitude." Efforts to convey radicalism through the conventions of sentimental fiction or to parry or parody the "pig" metaphor were only of limited success. More promising was the adoption of the chapbook genre by plebeian radicals such as Spence and William Hone. In the 1790s, however, a conservative publicist, Hannah More, made the most successful use of this popular literary form.

Confidence in the moral and political discrimination of a vernacular audience was far more visible after 1815. Its success could be measured in Cobbett's "Address to Journeymen and Labourers" in which the plebeian public was effectively dissociated from Burkean connotations of disorder and bestiality. Similarly, as Smith shows, the moral and political resources available to the vernacular were fully manifest in Hone's blasphemy trials, in which his defense was built on rhetorical strategies derived from Bunyan and Lilburne. The critical theoretical breakthrough had been made by Horne Tooke in his *Diversions of Purley*. Tooke's book eliminated the distinction between refined and vulgar language on the one hand and the equation of vulgar language with vulgar mind on the other. It provided the mainstay of a democratic, if ultraempiricist, theory of language. To have restored the political and intellectual centrality of Tooke to the radicalism of this period is one of the major achievements of Smith's book. Tooke's thought informed the preface to the *Lyrical Ballads*, captured the enthusiasm of James Mill, and stood behind the intellectual self-confidence of Cobbett and Hone. It represents perhaps the last common meeting point between forms of thought that developed into the divergent and antagonistic nineteenth-century traditions of plebeian radicalism, Tory romanticism, and utilitarianism.

By 1817, the year of Coleridge's *Biographia Literaria*, the alliance between radicalism and the literary intelligentsia was already over, and, interestingly, Tooke's name was consigned to oblivion by both Coleridge and Cobbett. A vernacular democratic public had been created, but, as this book makes clear, the process had been one of loss as well as gain.

This is an important and original book, and the implications of its argument are far reaching. The politics of language were not a peripheral component of the struggles of this period, they were central to them, and their reverberations were felt long after. Smith's cogently argued and well-written study is likely to change some of the basic questions asked about this formative period of English history.

GARETH STEDMAN JONES  
King's College  
University of Cambridge

NIGEL GROSS. *The Common Writer: Life in Nineteenth-Century Grub Street*. New York: Cambridge University Press. 1985. Pp. vi, 265.

JOEL H. WIENER, editor. *Innovators and Preachers: The Role of the Editor in Victorian England*. (Contributions to the Study of Mass Media and Communications,



number 5.) Westport, Conn.: Greenwood. 1985. Pp. xix, 335. \$35.00.

These two books recall long forgotten toilers in the nineteenth-century industry of literature. Nigel Cross investigates how the common writer stayed alive, given that only a tiny percentage of nineteenth-century writers were able to support themselves and their families decently by their pens. The essays in the collection edited by Joel H. Wiener ostensibly focus on periodical editors and their changing responsibilities, contexts, and influence. But, through the wealth of details each book contains about widely dissimilar individual careers, the reader discerns roughly the same story.

The story begins with a social group that was impoverished, that desperately attempted to maintain or achieve respectability, that largely lacked professional identity, but that often displayed a high degree of class consciousness. The story ends with increasing prosperity, as well as a new confidence in the respectability of "professional" men and women of letters. That very respectability, moreover, was accompanied by a double movement away from class identification: on the one hand, writers and editors decreasingly saw themselves as speaking for particular social classes; but, on the other hand, they increasingly saw themselves exclusively as producers of either "serious" or popular literature, categories that superseded traditional class distinctions. The mass markets that allowed literary producers to rise into the ranks of respectability were created by blurring class, regional, and even sexual distinctions within the readership—by creating the generalized reader who corresponded to the all-purpose professional writer. But professional writers then found themselves on the devalued side of a new hierarchical distinction: that between "artists" and mere writers.

To concentrate on beginnings and endings is to miss much of the richness of these two books, for their value is in their detailed evocations of literary lives in the middle decades of the century. *Innovators and Preachers* purports to focus on Victorian editors, but most of its fifteen essays are more broadly concerned with important figures in the history of Victorian periodicals. Indeed, as Wiener notes, the collection provides no consensus about what an editor is and "none of the essays deals specifically with the professional status of editors" (p. xiv). Nevertheless, his introduction also claims that "a pattern emerges from the careers of the editors discussed in these essays. It is of men and women overcoming the ambivalence of earlier decades and taking on full-fledged professional status" (p. xvi). A pattern of editorial professionalization indeed emerges but often as the obverse corollary of what is documented here: the difficulty that gentlemen and

women of letters had in fulfilling the function of editor. Robert Colby, for example, gives us informative accounts of Harrison Ainsworth, Charles Lever, Anthony Trollope, and W. M. Thackeray as periodical editors; Colby concludes that none of these novelists was truly a successful editor and that all found the work in some degree uncongenial. Ann P. Robson and John M. Robson tell a similar tale about J. S. Mill's frustrating attempts to find serious intellectual and political expression through the *London and Westminster Review*, and Barbara Quinn Schmidt's article on Leslie Stephens at the *Cornhill Magazine* also details the dissatisfactions of the genteel writer in the editorial role.

The career of G. W. M. Reynolds, lucidly presented here by Anne Humphreys, forms an interesting contrast to those of the genteel writer-editors. Reynolds had no scruples about giving his readership exactly what it wanted: a contradictory mixture of working-class radicalism, sensationalism, and advice about how to become respectable. Humphreys attributes Reynolds's success to his willingness to be as inconsistent as his readers and sees in his *Weekly* those paradoxical elements of the "popular mind" that allowed a class-oriented periodical press to transform itself into the "new journalism" of the last decades of the century, a journalism with a mass, standardized readership. This transformation made the working-class radicals and the gentlemen and women of letters equally irrelevant to the leadership of most periodical publications in the late Victorian period. Hence, although we never learn exactly what a professional editor is from this collection, we can see in a number of the essays the opening up of a space between writers, readers, and periodical proprietors that would be filled by such a functionary.

The functions of editor and writer underwent increasing differentiation during the course of the century as much because of the professionalization of writing as of editing. *The Common Writer* describes the sad plight of nineteenth-century authors before they had achieved a high degree of professional self-consciousness. Cross tells the stories of dozens of writers who struggled to eke a living out of authorship and who, despite frequent publication and even moderate popularity, were simply unable to support themselves. His primary source is the archives of the Royal Literary Fund, an organization that relieved destitute and deserving authors, and hence he perhaps unduly emphasizes the needy. Cross, however, presents convincing evidence that only a tiny percentage of authors were able to live off of authorship alone. In this respect, the recipients of the fund's grants were, then, typical.

The story of the fund is itself an instructive chapter in the history of the professionalization of authorship. Although the fund evolved into a char-

itable institution, it began as an organization to promote the professional interests of authors. Hence, it had, in its early years, somewhat contradictory aims: founded to do away with the need for patronage, it turned into an organ of patronage to take care of the immediate, pressing needs of distressed writers. Its original intention to reform the conditions of publication was eventually abandoned. Thus, it was sometimes reviled by authors such as Dickens, who saw it as a way of fostering in writers both habits of dependence and acceptance of inequitable treatment by publishers.

The antagonistic relationship between writers and publishers is a buried theme in this book. Cross assumes that publishers became rich while writers remained poor throughout most of the century, but we are given few details about the actual economics of literature. Rather, *The Common Writer* uses the records of the literary fund, among other materials, to construct capsule histories of the careers of writers who were never canonized. Cross sketches, for example, the young bohemians who surrounded both Dickens and Thackeray at midcentury, the working-class poets who sometimes achieved moments of fashionable celebrity before their patrons abandoned them to their former lives of obscure toil, and the "female drudges," who seem to have been the most shamelessly exploited denizens of Victorian Grub Street. Hannah Maria Jones, for example, whose enormously popular gypsy novels earned her publishers a fortune, was barely able to survive on the less than a penny-a-line she was paid. Moreover, when, in particularly bad straits, she applied to the literary fund for relief, she was rejected on the grounds that she was living with a man out of wedlock.

But, even in telling these pathetic stories, Cross keeps his attention on the private finances of the authors and declines to fill in the larger economic picture. Not until the last chapter of the book, when we learn of the Society of Authors' successful exertions to augment the writer's share of literary income in the last decades of the century, are we able to infer the enormity of the previous inequity. And only in that last chapter, which reports the growth of new kinds of journalism and fiction, does the rift between the "serious" and the commercial writer clearly emerge. Earlier in the century the bohemian "cockney" writers, who made a precarious living from such publications as *Punch*, had confronted the scornful amateurish gentility of the *Saturday Review* writers. But only in the last two decades of the century did commercial success itself become suspect, a sign of compromise and lack of integrity. An ethos of pure art developed in reaction to the new ethos of professionalization; ironically, the professionalism that gave commercial writers their first secure economic positions was immediately used to

differentiate them from the serious artists, the new breed of "uncommon writer."

CATHERINE GALLAGHER  
*University of California,  
Berkeley*

CYRIL EHRLICH. *The Music Profession in Britain since the Eighteenth Century: A Social History*. New York: Clarendon Press of Oxford University Press. 1985. Pp. 269. \$39.95.

Attractive to a readership well beyond historians specializing in music, Cyril Ehrlich's fresh analysis of a would-be learned profession illustrates some of the major problems of modern industrial society. The travail of the musical profession is frighteningly similar to the paper chase in graduate or professional schools—or even undergraduate colleges. As a result, musicians seem the paradigm of Marx's petite bourgeoisie: the harder they tried the less they got. Perhaps this sociological reality helps explain why the figure of the musician was so attractively Promethean to the myth makers of the romantic cultural revolution in the nineteenth century and to the youth revolution in the twentieth.

During the nineteenth century the music profession expanded in response to new employment opportunities for teachers and performers, a large proportion of whom were women. Demand was stimulated by many factors, such as the widespread desire for education, including music lessons, as well as by revolutions in commercialized entertainment, culminating in the silent cinema, which provided 80 percent of total musical employment by the 1920s. Then, just as new schools, examiners, and conservatories were producing the greatest supply ever of musical professionals, changing leisure patterns undermined the demand for teachers, and "talkies," followed by relentless improvements in recording technology, eliminated the need for large numbers of live musicians. Their unemployment during the Great Depression was worse than that of other professions.

Ehrlich breaks new ground with an amazing sum of logically assembled research. His literary skill makes the reader enjoy the procession of data and epiphanies, although he occasionally gives laundry lists of examples. These scholarly cadenzas might have been assigned to footnotes, which would make the text more attractive to the wide audience that it deserves.

The cultural history of music has become a lively interdisciplinary enterprise in recent years. Closest to Ehrlich's work is the equally thorough research by William Weber. Speculative sociology on related topics is found in the writings of Theodor Adorno. The most readable sociology of music in the past two



centuries remains Arthur Loesser's *Men, Women, and Pianos* (1954).

Although Britain was an especially hard case for musicians, Ehrlich's observations are sobering. They apply all too well to other professions in which high aspirations entice young people but scant prospects are available after so much effort.

CONRAD L. DONAKOWSKI  
Michigan State University

DEBORAH M. VALENZE. *Prophetic Sons and Daughters: Female Preaching and Popular Religion in Industrial England*. Princeton: Princeton University Press, 1985. Pp. xvi, 307. \$38.50.

We are indebted to Deborah M. Valenze for her wise and sympathetic interpretation of a passage of cultural history that is little known or understood outside the narrow confines of denominational studies. Convinced that popular religion would be a "sensitive indicator of economic and social influences on laboring life," Valenze sought to reconstruct "the sacred world view" of village laboring families who found themselves in the front lines of the Industrial Revolution (pp. 5-6). She studied several plebeian evangelical sects that flourished between 1790 and 1850, such as the Primitive Methodists and Independent Methodists—offshoots of the Wesleyan revival, which also had affinities with seventeenth-century dissent and touched deep currents of popular piety and folk belief. Valenze set herself a two-fold task: first, to establish the connections between sectarianism and economic change and, second, to delineate and account for the widespread phenomenon of female preaching within the sects.

Her argument rests on a number of closely detailed case studies, among which are illuminating chapters on Belper, a Derbyshire town that supported itself in the eighteenth century by nail making and other cottage industries and was transformed after 1800 by the introduction of large-scale cotton mills, and Filey in Yorkshire, a fishing village whose economy was drastically restructured by the demands of the mushrooming urban market. In such places immigrants from the countryside responded readily to the message of sectarian preachers who, Valenze argues, offered a nostalgic and idealized countercultural alternative to the values of early capitalism. Within the circle of cottage prayer meetings converts found a reassuring echo of traditional verities being torn asunder by forces beyond their control. The author sees cottage revivalism as an analogue to the domestic system of production and sectarian theology as an impassioned response to the destruction of the artisan household as a social and economic unit.

For Valenze the receptivity of sectarianism to female prophesying was a function of its status as "religion of the domus" (p. 209). Organized around intimate and informal gatherings for prayer and testimony, the provision of assistance in time of hardship and illness, and concern with the instruction of children, the sects provided a congenial environment for hard-pressed women and a platform for their eloquent complaints.

Any historian seeking to decipher the social and spiritual messages of evangelical sectarianism must be frustrated by the sparseness of evidence for the content of preaching at the village level. We have virtually no documentary record of the nature of the message of the female preachers whose lives Valenze has expertly pieced together from brief, scattered, and often tendentious memoirs. Thus, Valenze's superb chapter on the female revivalists of Leeds, who flourished under the leadership of preacher Ann Carr in the 1820s and 1830s, makes a valuable contribution to our understanding of female evangelism. Valenze provides a close reading of the hymnal developed by the community for its own use and extracts from it the outline of a theology speaking directly to displaced and impoverished laborers, and especially to women. The revivalists vividly conveyed to their impoverished flock a message replete with criticism of earthly inequities and the anodyne teaching of the Established Church.

Although eager to see sectarianism in an elegiac light, Valenze is nevertheless aware that she is dealing with a complex and volatile phenomenon. She shows that itinerant preachers followed their flocks to the cities and adapted their message to rapidly changing realities of industrial life. But, in her concern to demonstrate the backward-looking nature of sectarian ideology, Valenze is inclined to overlook fissiparous and centrifugal tendencies inherent in the movement from the start. In its encouragement of untrammelled itineracy, antiauthoritarianism, and the gospel of self-improvement, religious sectarianism could challenge, rather than reinforce, family discipline and village solidarity. Valenze does not directly confront the arguments of Alan Gilbert that evangelical dissent, in all its forms, held a special appeal to the upwardly mobile, that is, to the socially and economically adventurous. A movement placing dual emphasis on personal experience of God's grace and the fellowship of believers within a tightly structured community set up a fascinating and perplexing tension between individualism and mutuality.

Valenze concludes, somewhat abruptly, with a brief afterword, chronicling the decline of cottage-based popular evangelism and the near eclipse of working-class female preaching. At this point an assessment of the weight and influence of sectarian-

ism in laboring communities—either in terms of numbers, or in comparison with competing cultural forces (Anglicanism, Wesleyanism, or radical political movements, for example)—would have helped relate Valenze's exhaustive and innovative research to the lively controversy about the role of religion in the process of class formation begun so explosively by E. P. Thompson and carried forward recently by David Hempton and others.

Valenze's work dramatically widens our angle of vision and will be of lasting value to every scholar interested in the origins of modern society. That many of the issues she raises remain open for debate in no way diminishes her very considerable achievement.

GAIL MALMGREEN  
University of Massachusetts

DUDLEY BAINES. *Migration in a Mature Economy: Emigration and Internal Migration in England and Wales, 1861–1900*. (Cambridge Studies in Population, Economy, and Society in Past Time, number 3.) New York: Cambridge University Press. 1985. Pp. xiv, 354. \$44.50.

Reportedly, 44 million emigrants left Europe between 1821 and 1915; of these, 10 million left Britain. British emigration peaked between 1870 and 1914. Nonetheless, the literature on neither side of the Atlantic gives to British emigrants attention commensurate with their numerical importance. In his excellent study Dudley Baines redresses this imbalance.

Baines begins by setting his study in the context of European migration studies. The body of the book presents his findings concerning origins of emigrants, return migration, the surge of emigration in the 1880s, the relationship between emigration and urbanization, stage emigration, and the putative uniqueness of Welsh migratory patterns. He argues that late nineteenth-century emigrants were likely to have been born in cities and that therefore only a minority emigrated in direct response to the agricultural distress of the 1880s. About 40 percent of British emigrants returned; these returns were facilitated by steamship travel. Baines found that internal migration—particularly migration to cities—was not a substitute for emigration, as scholars often assert. From the consistency of patterns of emigration by county, Baines concludes that encouragement from previous emigrants was most crucial to the decision to move. Finally, he finds that migrations of the Welsh did not follow a unique pattern.

Baines's study points to the complexity of migratory behavior and to the variety of migration streams that existed alongside the rural-urban movements of the nineteenth century that have

absorbed scholarly attention. For example, natives of cities emigrated abroad and also left their home city for other areas in Britain. Emigration and migration within the British Isles seem to have involved different decisions and different people. Temporary emigration, calculated in advance, was common. Finally, information, passed by letter and by returned emigrants, was an important determinant of migration.

These are remarkable conclusions to draw from the data Baines possesses, because no single source describes all the emigrants from Britain. The study's major task is the creation of a portrait of emigrants by county of origin and gender; this portrait is inferred from census data in combination with birth and death figures; these sources are used to estimate net internal migration from county to county and the net number of overseas emigrants. Calculations of both figures—a formidable task—were carried out for the counties of England, Scotland, and Wales. Data on net migration at the county level are prone to error, given the problems of estimating mortality and age at migration. Moreover, they provide incomplete information about home areas because many counties included both agricultural districts and substantial towns; as a consequence, origins of migrants can rarely be identified with certainty as urban or rural. Finally, net migration figures and net emigration figures reveal little about the frequency, timing, motivation for, or process of migration; they yield only the net effects of geographical mobility.

The author's care with his analysis, methodology, and logic inspire confidence in his conclusions. Baines is keenly aware of the margin of error for which he must allow and of the controversial nature of some of his conclusions. Appendixes record the analyses in detail, and the text carefully leads the reader through the implications of the findings.

Internal migration receives cursory treatment because the county-level data are limited and because the author's primary interest lies in identifying emigrants from England. Nonetheless, Baines's study convincingly challenges received wisdom about both internal migration and emigration; this fine general work should inspire cross-sectional and case studies to answer the many questions it raises.

LESLIE PAGE MOCH  
University of Michigan,  
Flint

GERTRUDE HIMMELFARB. *Marriage and Morals Among the Victorians*. New York: Alfred A. Knopf. 1986. Pp. xiv, 253. \$19.95.

Gertrude Himmelfarb has become that rarest of entities, a historian who thinks before she writes and

whose prose demands that her reader rethink long-cherished positions. Like the Victorian intellectuals whose works she has illuminated for over three decades, Himmelfarb has in this volume, with one major exception, republished review essays of the past decade that are usually more important and virtually always more elegant than the works on which she is commenting. The value of these pieces lies less in the new information conveyed, for there is little, than in the new insights on generally familiar topics.

These essays are urbane and sensitive even when they arouse disagreements with their arguments or conclusions. Writing as an engaged historian with an acknowledged moral and political stance, Himmelfarb is absolutely forthright in her opinions and judgments. She is also more often than not more nearly on target than most of her readers are likely to admit. Historians who bring an ideological double standard to the treatment of women and men in Victorian marriage stand condemned. Political thinkers such as Jeremy Bentham and William Godwin who would redesign the world without regard to feeling, imagination, common sense, and liberty emerge from Himmelfarb's prose in all the ultimate banality of their utopianism. The article on Bentham's plan for poor relief, a reprint of an original rather than a review essay, continues the author's longstanding crusade against Benthamite social planning. Himmelfarb also does not permit later Fabian social planners such as Beatrice Webb and Sidney Webb to escape the serious doubt about their good judgment and good intentions that must necessarily arise from their public enthusiasm for Stalin. The world of Bloomsbury, so frequently presented as a realm of happy relief from the alleged confines of Victorianism, stands censured not for being uninhibitedly sexual and freethinking but for pursuing sexuality without loyalty, love, or even vague good manners. Himmelfarb also makes thoughtful critical comments on another feature of Bloomsbury that is usually so carefully ignored by scholars: its contempt for the working class and for democracy.

Yet Himmelfarb does not write merely as a cultural censor. She admires William Blackstone, Edmund Burke, Thomas Macaulay, and Benjamin Disraeli, who emphasized the role of historical continuity and social complexity in personal and political identification and development. She also has warm praise for Michael Oakeshott. The virtue of all her heroes is their commitment to liberty and their skepticism about the ease of its achievement and preservation. They stood as intellectual rocks in a stream of liberal thought that rushed onward often without considering ultimate consequences for the quality of human life. Likewise in these essays Himmelfarb stands as a powerful intellect

who exposes the glibness of much historical analysis that would ignore the complexities, ironies, and paradoxes of the human situation. By thus going against the grain, Himmelfarb brings us to novel insights into the Victorians and valuable insights into ourselves as creatures of moral capacity.

FRANK M. TURNER  
Yale University

JANE LEWIS, editor. *Labour and Love: Women's Experience of Home and Family, 1850-1940*. New York: Basil Blackwell. 1986. Pp. xi, 274. Cloth \$49.95, paper \$14.95.

No one interested in social history, women's history in particular, should miss reading this fine collection of essays. It is designed to complement another volume, edited by Angela John and titled *Unequal Opportunities: Women's Employment in England, 1800-1918* (1985). The overarching theme is that, given women's special roles as daughters, wives, and mothers, the paid employment of women outside the home cannot be understood in isolation or separate from women's status and work within the home. Several of the contributors to the volume under review have already made their mark as feminist scholars and here draw on their larger works already published: Carol Dyhouse on middle-class mothers and daughters, Jane Lewis, who also served as editor of the collection, on working-class wives and mothers confronting state intervention in family matters, Elizabeth Roberts on women's strategies in coping with inadequate family incomes, and Diana Gittens on women's marital status, paid work, and kinship ties in a small Devonshire textile town. Other contributors make their scholarly mark now by presenting some of the findings contained in their graduate theses, which, one hopes, will soon be published in full: Lynn Jamieson on working-class mothers and daughters in urban Scotland, Barbara Brooks on women and reproduction, Dina Copelman on married women teachers in London, and Pat Ayers and Jan Lambert on domestic violence among the working classes of Liverpool. Finally, two contributors' essays are in a sense progress reports on research projects: Lucy Bland's study of middle-class women and marital sex and Ellen Ross's examination of working-class mothers in London (my particular favorite). These are all important subjects that have only recently come under scholarly investigation.

The essays here are solidly grounded in research done in primary sources of various and sometimes novel kinds and in the best and most recent secondary works, and several make fascinating use of oral history as well. Some of the essays could be more clearly focused, and some could be less repetitious;

but on the whole the collection is well written, and it contains a wealth of interesting information and many keen insights. One could quibble about the book's structure. The essays average little more than twenty pages in length, so their division into separate sections with subheadings seems cumbersome and sometimes a bit arbitrary. Likewise, the grouping of the ten essays into five separately titled parts is unnecessary and in a sense runs counter to the main theme—that the total fabric of women's lives needs to be considered, not one aspect of their lives apart from the others. But this is merely a quibble. On the whole I have the greatest admiration for the work of the contributors to this book, which illustrates so well the vitality of women's history, and I look forward to seeing the fruits of their future endeavors.

LEE HOLCOMBE  
University of South Carolina,  
Spartanburg

ELIZABETH ROBERTS. *A Women's Place: An Oral History of Working-Class Women, 1890–1940*. Reprint. New York: Basil Blackwell. 1985. Pp. vii, 245. \$12.95.

Elizabeth Roberts's oral history of working-class women from 1890 to 1940 consists of extracts from interviews with about one hundred sixty people who live in the northwest of England in three Lancashire towns: Barrow, where over a third of the workforce were employed, during these fifty years, by Vickers, the shipbuilding and heavy engineering firm; Preston, mainly a cotton town with docks; and Lancaster, the county town, where the major industries were the manufacture of oilcloth and linoleum.

Roberts admirably succeeds in giving the true flavor of working-class women's lives, not only through their own speech but also through much of her commentary. She brings out what many working-class and women's histories omit, the happiness and pleasures of these women's lives—the dances, pierrot shows, courting, kind as well as harsh employers, and the love within families. "I was happy somehow" says Miss Ainsworth of her life when, aged twelve, she began work as a weaver (p. 59). Nevertheless, Roberts evokes the harsh discipline children had to learn in families with limited resources determined not to fall into destitution. She does not shirk women's cruelty to each other—to one who bore an illegitimate child (p. 78) or to one having only a single child (p. 103).

Interspersed among all the lively material are fumbling links, gauche sentences, and useless rhetorical questions when her graphic answers would suffice (for instance on birth control and jobs [p. 100]). A section entitled "Were Women Successful Managers?" proceeds for five lines before she

reaches the central point of the importance of a husband's ability to supplement family income by "living off the land." The scattering of platitudes and obvious remarks, such as "there has been evidence of both change and continuity in the lives of working-class women" (p. 202), irritatingly interrupts the flow of the book. These small blemishes, however, do not diminish the value of this perspicacious and refreshing addition to the history of British working women.

SHEILA LEWENHAK  
University of London

ROBERT GRAY. *Cardinal Manning: A Biography*. London: Weidenfeld and Nicolson. 1985. Pp. 366. £16.95.

With the partial exception of Shane Leslie's work (1921), Cardinal Henry Edward Manning (1808–92), England's Roman Catholic primate during the last twenty-seven years of his life, has not been well served by his biographers. Edmund Purcell's supposedly authorized two-volume life (1895) of the eminent convert persuaded "the English establishment . . . that the man who had consistently shown up their selfishness and indifference through his work for the dispossessed was, after all, simply a power-hungry hypocrite" (p. 5), and Lytton Strachey's poison pen-portrait in *Eminent Victorians* (1918) confirmed that verdict. Since 1945, when Alphonse Chapeau began the arduous task of salvaging and sorting what remained of Manning's papers, a comprehensive life has been awaited. Robert Gray's is not that work, but it is a generally thoughtful and well-balanced biography that convincingly sets Manning in his world. Although the book is based entirely on printed works, Gray's endnotes remind us of the plethora of relevant published primary sources now available.

In essentially chronological manner, Gray recounts the story of the youngest son of a wealthy West Indies merchant, banker, and Tory M.P., whose hopes for a political career after Harrow and Oxford were dashed by the collapse of his father's business. His promising clerical career within the Church of England was blighted by the doubts the Oxford Movement aroused that a body so entangled with the state and so divided in its theological claims could constitute the one true Christian church. Manning's conversion to Rome was followed by an alleviation of theological doubts but by no cessation of self-questioning on other grounds. Gray is right to suggest that Manning was less single-minded in his ambitions than Strachey implied. Not all his actions between 1851 and 1865 seemed likely to lead to higher office. In his early years as a convert, Manning conceded, "I have consciously offended



Protestants, Anglicans, Gallican Catholics, national Catholics, and worldly Catholics, and the Government and public opinion in England" (p. 199). Pope Pius IX nevertheless named him archbishop of Westminster in 1865 and cardinal a decade later.

Gray sees Manning in that office as a dogmatic theologian (the prime champion of "papal infallibility"), an efficient administrator, and a dedicated social reformer—"the most accomplished and benevolent man of affairs that the Church has possessed in England since the Reformation" (p. 210). For Manning his prime task was not to convert additional English aristocrats but to educate and nurture hundreds of thousands of recent immigrants from Ireland. Manning became enough of a "Mosaic radical" or "socialist" to cause his portrait to be displayed with that of Marx at London's 1890 May Day parade. Gray concedes that Manning developed no consistent philosophy on matters of public policy. Thus, he could describe "the law of property" as "founded on the law of Nature" (p. 303) yet urge authoritarian state intervention in the form of factory acts, rent control, public works for the unemployed, and the abolition of pubs. An advocate of the relief of Irish grievances, he scarcely mourned the defeat of the Home Rule Bill of 1886 because it would have meant the removal of most Roman Catholic members from the imperial Parliament. A cold, austere ascetic in appearance, he supported the cause of the poor with all the evangelical fervor of his Anglican father, "a perfect daemon of philanthropy" (p. 11).

Gray makes greater allowances for Manning's sometimes fanatical pronouncements than for similar outbursts by representatives of English anti-Catholicism, "a weird and unpleasant national psychosis" (p. 101). He also seems unfamiliar with recent secondary works on many of the public issues with which Manning dealt. Yet the book remains an excellent introduction to a fascinating man for whom the Victorian era was characterized not by calm but by apocalyptic religious, political, and social strife.

WALTER L. ARNSTEIN  
University of Illinois

JONATHAN ROSE. *The Edwardian Temperament, 1895-1919*. Athens: Ohio University Press. 1986. Pp. xiv, 275. \$26.95.

As Alfred Havighurst wisely remarked in *Britain in Transition: The Twentieth Century* (1979), "Edwardian thought was a complex of ideas; no two students today would reconstruct it in the same way and with the same results." It is possible to question Jonathan Rose's decision in this study to concentrate solely "on those Edwardians who produced innovative and

lasting creative work, who continue to interest us today as artists, writers, and thinkers" (pp. xii-xiii). One could regret the absence of luminaries such as Marie Lloyd or desire a vision of the Edwardian temperament that encompassed the inhabitants of Robert Roberts's *Classic Shm* (1971). But that would be to quibble with Rose's right to reconstruct Edwardianism as he sees best, and his best has produced a perceptive, valuable book.

Rose stresses the Edwardian quest to replace the lost certainties of faith with a variety of surrogate secular religions, which sought to eliminate the "boundary between the sacred and the mundane" (p. 17). Whether the Edwardians were exploring psychic phenomena, preaching the Social Gospel, cultivating human relations, worshiping life, or glorying in the pursuit of fun, Rose argues that they were, for the most part, in search of connectedness—the reconciliation of opposites within themselves and society. Rose points out the exceptions to this tendency but, nonetheless, perceives it as the central thread in the intellectual output of the years 1895-1919. He persuasively contends that this emphasis on unity and harmony frequently allowed important Edwardian thinkers to deceive themselves and embrace illusory connections rather than face tough choices.

Periodization and proof—problems for any historian—are perhaps most troublesome for the intellectual historian. Rose tackles periodization by widening the time frame by several years on either side of the reign of Edward VII. His claim that intellectual developments between 1895 and 1901 more closely resembled those of the early twentieth century than of the late nineteenth is justifiable, but it does not solve the difficulty. Much that occurred in the late 1890s dated from the 1880s or earlier (for example, fascination with psychic research, the influence of T. H. Green's idealist philosophy, or the reaction against fatalism). In fact, late Victorian thought merged imperceptibly with Edwardian, and any starting point for Edwardian thought is bound to raise objections. Similarly, much that supposedly ended with World War I continued, as in the work of E. M. Forster.

As for proof, the accumulation of quotations to support a general argument may not be the most satisfactory way to sustain a point of view in intellectual history. An equal number of quotations may always be marshaled in opposition. Ultimately, the monotony of its organization keeps Rose's work from being as stimulating as Samuel Hynes's *The Edwardian Turn of Mind* (1968). Rose's book is divided into short segments, each beginning with a general statement followed by several pages of illustrative examples. Yet these examples, all aptly chosen and engagingly described, nonetheless contrib-

ute to a coherent, if partial, perspective on Edwardianism.

JANET OPPENHEIM  
American University

MICHAEL FREEDEN. *Liberalism Divided: A Study in British Political Thought, 1914–1939*. New York: Clarendon Press of Oxford University Press. 1986. Pp. x, 399. \$49.95.

Michael Freeden quite properly won great interest and considerable admiration for his *The New Liberalism* (1978)—an analysis of “an ideology of social reform” during late Victorian and Edwardian England. That book raised many important questions for political scientists as well as for historians. The first objective of historians, it may be argued, is to understand people during a given period of time. But Freeden suggests that he knows more about political and social thought than did the people themselves. He concludes that the “New Liberalism” was the only important stream of thought in the years of his study, a position that might be contested, and brushes aside divisions among the Liberals themselves. Often his point of view is balanced. He agrees that legislation does not occur in a vacuum but is convinced that ideas are facts. *The New Liberalism* has led to reexamination of our knowledge and understanding of the period.

This background is essential to assessing Freeden’s new book. It too arrests without always being convincing. But it breaks new ground at many points. Freeden divides liberal thought between “left-liberalism” and “centrist-liberalism,” and he asserts that both tendencies have had considerable influence outside the Liberal party.

On the defensive in a period when new solutions to problems had to be found, liberalism was not dormant, nor was its path one of unmitigated failure. Freeden adds greatly to our understanding of the impact of World War I. The chapter “The Worker as Citizen” underlines the new status of labor after 1918. The relation of liberalism to the Labour party is systematically examined; the role of the trade unions raised the most serious problems. And for the first time we have a detailed study of the Liberal Summer School, “the powerhouse” of liberalism, in the 1920s. Was John Maynard Keynes a liberal? The question, discussed at length, remains unanswered. Freeden terms the 1930s “a decade of dormancy,” in which democratic ideals were challenged. But planning, a concept that defies precise definition, provided, to a degree, a connecting link between various groups still called “liberal.” In the Beveridge Report (1942) the author finds a blend of leftist and centrist liberalism.

This review has sought to emphasize major points. I hope that it will lead both historians and political scientists to a careful reading of an important book.

ALFRED F. HAVIGHURST  
Amherst College

GWYN MACFARLANE. *Alexander Fleming: The Man and the Myth*. Cambridge: Harvard University Press. 1984. Pp. xiv, 304. \$20.00.

Occasionally, reviewers are rewarded for reading books on subjects that seem to have been overly scrutinized by historians. Gwyn MacFarlane’s biography of Alexander Fleming provides a surprisingly fresh look at the roles of Fleming and the co-winners of the Nobel Prize awarded in 1945 for the discovery of penicillin, Howard Florey and Ernst Chain. The book is a well-written, finely crafted historical account. MacFarlane provides extensive evidence to show that Fleming received the most attention and more credit than he had earned for his role in the discovery and application of penicillin. MacFarlane also succeeds in making readers feel that Fleming is a person they have known. By the end of the volume one believes that the history of penicillin has been revised and, even more, that the history of medical science has come of age in the sense of skillful delineation of primary and auxiliary focuses, discussion of evidence, and understanding and evocation of the setting of scientific and medical events.

Alexander “Alec” Fleming (1881–1955) was an unprepossessing Scottish biologist who frequently earned top grades in his classes and won many academic prizes, who studied medicine without taking the final examinations for the medical degree, and who became a fellow of the Royal College of Surgeons without practicing surgery. His consuming passion was playing games of various sorts, including the more physically demanding billiards, croquet, tennis, and games of mental manipulation, at which he frequently conquered his opponents. His playful manner carried over into the laboratory, where he painted pictures on petri dishes using molds of various colors in place of the artist’s usual oils or water colors. Fleming never claimed to be a genius or a great discoverer and MacFarlane agrees with his self-evaluation.

Shortly after the turn of the twentieth century, Fleming embarked on his career as a bacteriologist in Almroth E. Wright’s department at St. Mary’s Hospital in London and stayed there for forty years. Wright dominated and enervated his staff and provided a conceptual basis for Fleming’s work on penicillin and other drugs.



Describing scientists and medical actions is an exceptionally difficult task if the writer is interested in more than retelling what happened in the laboratory or hospital. The historian must seek out evidence in addition to technical reports and publications devoid of motivational and emotional statements. MacFarlane has assembled his sources, mostly primary, and used his personal knowledge of the institutions and some of the protagonists in this history.

The author understands his many sources, which include his knowledge gained as a student and, later, as professor of clinical pathology at the University of Oxford. His interpretations sometimes contradict comments made by the significant figures in the history of penicillin. For instance, MacFarlane writes: "By Nov., 1938, Florey, who had kept in close touch with Chain's work, was beginning to be excited about penicillin too. Though they both insisted later that their interest at that time was purely scientific, and that the possible therapeutic value of penicillin was not considered, there can be little doubt that such a possibility was at the back of their minds then and very much in the forefront a few months later" (p. 170). The author then proceeds to build his case that Florey had other attributes required to develop penicillin as a dependable drug: "Florey was not only a hard worker and a clever scientist, he was a great organizer. He had the ability to recognize and to use the relevant special talents of his colleagues and assistants, and he had a very special quality of his own, the ability to inspire the confidence and enthusiasm of a group of experts so that they became a very effective team under his leadership" (p. 170). In 1939, with no money for research and the war approaching, Florey decided to gamble all his resources on penicillin, "a dark horse at best."

Thus, we learn of MacFarlane's belief that Florey was an equally, if not primarily, essential figure in the development of penicillin as a therapeutic agent. We also understand that medical discoveries are complex, socially, politically, and economically controlled events that transcend any one person's abilities, intentions, or achievements.

MacFarlane comments briefly on the success of Americans in quickly learning to produce penicillin in large quantities and the failure of the British in this regard. He writes that the British did not want to patent their work, and the Americans did. Of course, Fleming had nothing to patent. This problem is more complex than a question of patent rights, and MacFarlane's answer is not satisfactory. In the meantime John C. Sheehan's book, *The Enchanted Ring: The Untold Story of Penicillin* (1982), explains his role in the synthesis of penicillin, a

necessary step in the large-scale manufacture of penicillin.

AUDREY B. DAVIS  
National Museum of American History  
Washington, D.C.

HARRIE MASSEY and M. O. ROBINS. *History of British Space Science*. New York: Cambridge University Press. 1986. Pp. xxi, 514. \$89.50.

Space science, as distinct from astronomy, is almost entirely a post-World War II discipline, largely dependent on the technologies of the rocket, transistor, and digital computer. Previously, instruments carried in balloons could record phenomena at altitudes of thirty kilometers above the earth's surface; now, rocket-borne instruments could explore up to one hundred and fifty kilometers, and satellites would enable regular surveillance of the region above one hundred and fifty kilometers. We are all familiar with the dramatic events in space exploration, including the launching of the first earth satellite, Sputnik 1, the journeys to the Moon, the Voyager 2 spacecraft, now far out in the solar system beyond the planet Uranus, and, of course, the tragic fate of the space shuttle Challenger.

In the United States the National Aeronautics and Space Administration (NASA) has, in spite of its youth, undertaken to have its history recorded and for this purpose has set up a history office that has the assistance of the American Historical Association and that contracts with historians to write accounts of various aspects of NASA's history, the goal presumably being a comprehensive set of histories. (See the recent booklet, *History of NASA: The NASA History Office* [1986]). No similar development has yet occurred in the United Kingdom, and Harrie Massey and M. O. Robins are not historians but scientists who had active, and, in Massey's case, prominent, roles in the story they chronicle: the development of space science in Britain from 1953 to 1983. Although the book has limitations, including the excessive attention given to administrative matters, it breaks new ground in providing a survey of the development of British space science and its relationships with U.S., European, and Commonwealth space science. In spite of the book's inadequate documentation (for example, chapters 4 and 6 are undocumented, and chapter 8 has only one footnote), it should be of considerable use to future historians of British space science. Personal memoirs by the authors might have been even more useful; the account they have presented contains little on individual personalities.

The book is wider in scope than its title suggests. Developments in space science in other countries influenced developments in Britain, and chapters

are devoted to evolving European organizations for cooperation in space research and to Commonwealth cooperation. In addition to these international administrative aspects of space science, accounts are given of the scientific studies conducted by British space scientists, including those on the figure of the earth, the neutral atmosphere, the ionosphere, the magnetosphere, and cosmic rays. Although I suspect that this volume will not appeal to the majority of *AHR* readers, it should prove valuable to anyone interested in the history of space science. It places demands on the reader, not least of which is to keep the ninety-odd acronyms straight in one's mind. The book is nicely illustrated, however, and, as one would expect, solid on scientific and technical matters.

WILLIAM MCGUCKEN  
*University of Akron*

MICHAEL MACCARTHY-MORROGH. *The Munster Plantation: English Migration to Southern Ireland, 1583–1641*. New York: Clarendon Press of Oxford University Press. 1986. Pp. x, 318. \$53.00.

The plantations in Ireland are usually dated from the mid-1550s, but they emerged in full flower only in the 1580s. The ambush in November 1583 of the rebel fourteenth Earl of Desmond led to the escheat to the crown of six hundred thousand acres of scattered lands in the southern province of Munster; despite six years of upheaval and war from 1598 to 1604, by 1641 the resulting plantation and its associated towns seem to have attracted some twenty thousand English immigrants. D. B. Quinn called attention to the importance of this episode in an oft-quoted article published in 1966 (*Journal of Cork Historical and Archaeological Society*), and Michael MacCarthy-Morrogh now provides the first scholarly book-length study.

Unlike the early seventeenth-century plantation of Ulster, which aspired to sweep out former Irish occupants and repopulate six of the province's nine counties, the Munster plantation was limited to what were said, or thought, to be the Earl of Desmond's former lands. Because these were concentrated in the eastern and southwestern sections of the province, numerous other areas were virtually unaffected.

MacCarthy-Morrogh's focus is the plantation area rather than the province as a whole. The picture presented is one of success, rather than of failure, of estate building and economic development rather than of expropriation and exploitation. The original inhabitants, whether Old English or Gaelic, are not viewed as victims of Sassenach intrusion but as generally passive observers of a process that brought them little immediate harm. He describes the

Munster plantation as a West Britain, a veritable extension to southern Ireland of the enterprising culture and economy of the English West Country.

This is a fresh and stimulating interpretation, and it compels reconsideration of the nationalist version of plantation as mere confiscation and glorified theft. MacCarthy-Morrogh is not alone in his revisionist stance. In a study of the greatest of Munster planters, Richard Boyle, first earl of Cork, (*The Upstart Earl* [1982]), Nicholas Canny vigorously defended his subject from three centuries of obloquy, and two recent books on the Ulster plantation by Phillip Robinson (1984) and Raymond Gillespie (1985) treat colonization in a clinical and nonjudgmental fashion. Whom, and what, is to be believed?

As regards Munster, MacCarthy-Morrogh's study will not easily be faulted for its narration of events. The first five of eight chapters tell the story in admirable detail. The one map is inadequate (p. 90), but the scholarly apparatus is otherwise generous and provides a wealth of corroborative information. In the Rankean sense of definitive work, the Munster plantation has now been done.

Interpretation is another matter, and here, especially in three lively concluding chapters on motives for emigration, the economy, and the state of the plantation in 1641, there is room for controversy. Although generally well written, thoughtful, and cosmopolitan, the book shies away from some urgent questions. If it is congruent in many respects with the image of Munster projected by Canny, it declines to comment on his provocative, and interesting, suggestions. MacCarthy-Morrogh cites (as did Canny also) Terrence Ranger's unpublished doctoral thesis (Oxford, 1958) on the Earl of Cork, but he avoids discussing the unflattering picture Ranger presents of this successful planter's land-grabbing schemes.

Finally, by emphasizing the success of the plantation, MacCarthy-Morrogh ignores an inherent flaw in its foundation that every late twentieth-century reader must want, and need, to understand. Admittedly, some of these matters could be the subject of another study by the author or someone inspired by him, and undoubtedly the ways in which the influx of Englishmen caused late sixteenth-century Munster to flourish, commerce to thrive, and agriculture to prosper needed to be appreciated and set forth. But the author tends to neglect ways in which discord was sown and an alien population intruded into an unwelcoming land. Despite these reservations, this book makes a major contribution to the history of early modern Ireland and the English connection.

KARL S. BOTTIGHEIMER  
*State University of New York,  
Stony Brook*

ELIZABETH MALCOLM. *"Ireland Sober, Ireland Free": Drink and Temperance in Nineteenth-Century Ireland.* (Irish Studies.) Syracuse: Syracuse University Press. 1986. Pp. xii, 363. \$27.50.

The idea of consumer sovereignty, that is, the right of individuals to purchase whichever goods they like as long as it does not interfere with others, has always been taken more seriously by economists than by other people. A good case in point is the ubiquitous demand for mood-altering drugs. Even a casual look at historical evidence shows that the desire for commodities such as wine and coca leaves is almost as old as civilization. Equally obvious is that some people have always looked askance on this habit. Why the intoxication of others has given rise to such moral indignation is not clear, but the antipathy to drugs and alcohol is also quite old. The current "drug crisis" campaign, fanned by the publicity generated by our First Lady and *Newsweek* magazine is one more example, as is Mikhail Gorbachev's conviction that vodka is the main reason why the socialist experiment has failed. Most temperance crusaders share a disdain for systematic evidence and have no interest in rational ordering of social priorities. Temperance advocates usually are permeated by a pious knowledge that the campaign against a drug is just and good regardless of the facts. The abuses of a few are invariably mustered as irrefutable proof of the pernicious effects of a habit on society at large. Mood-altering substances, from alcohol to cocaine to amphetamines, are one of the most useful scapegoats ever invented.

Nineteenth-century Ireland needed a scapegoat. It was desperately poor, politically repressed, religiously divided, abandoned by millions of emigrants, and ravaged by the Famine. After 1850 changes in marriage patterns, when tens of thousands of young men and women were forced into celibacy, led to much personal frustration. The temperance movement that emerged in Ireland in the late 1820s regarded alcohol as the root of all evil. Moderation and, preferably, total abstinence became, in the temperance propaganda, the keys to the Promised Land of prosperity and freedom.

In this detailed and well-researched book Elizabeth Malcolm has written the definitive history of this movement. At first, the temperance campaign was run by Presbyterian clergymen whose tactics were based on moral persuasion. In the early 1840s the legendary Father Mathew, a mesmerizing Roman Catholic friar, persuaded hundreds of thousands of frenzied followers to take a pledge of abstinence. Yet the movement failed, and Father Mathew, in spite of his charisma, turned out to be of little lasting importance.

The failure of example and reason to alter Irish drinking habits made the righteous men and

women of the temperance movement turn to Westminster. In a well-orchestrated political lobbying campaign, the temperance movement engaged in an attempt to have drinking severely restricted in Ireland by law: Sunday closing and "permissive" laws (which would allow local authorities to close pubs) were repeatedly put on the agenda of the House of Commons. This tactic, in spite of some temporary successes, proved equally fruitless in the long run. Publicans' lobbying and the justified fear that widespread prohibition would spark anger and unrest prevailed. A few voices of reason, such as that of the Home Ruler William O'Brien, who pointed out that "the lives of the Irish people were sufficiently joyless already" and that Parliament would be better advised to try to improve the condition of the Irish people than to "debar them from one of the few enjoyments they had," must also have had their effect. Toward the end of the century the temperance movement allied itself with Catholic nationalism and Gaelic revivalism through the work of the Jesuit Paul Cullen. Advocates of temperance allied with religious fundamentalists as easily as with cultural nationalists.

Malcolm's book is both impressive and exasperating. She describes in detail the political actions of the temperance societies and the personalities who led them. Yet she rarely digs beneath the surface: What were many of these temperance advocates really after? Was there any truth in their allegations? Was drinking just a convenient scapegoat or a genuinely harmful phenomenon? And what about social institutions correlated with but not identical to alcohol consumption, such as fairs, wakes, and taverns? Malcolm shows that Irish levels of alcoholism approximated those of other countries and that consumption habits changed significantly during the nineteenth century—especially the shift from spirits to beer after 1850. Most of these shifts can, however, be explained by changing economic conditions and were not directly linked to temperance. Whether the antidrink movement had a significant long-term effect on alcohol consumption is, of course, hard to determine. Malcolm suggests that it had some effect; some people who might otherwise have indulged in inebriating drinks became teetotalers. Yet alcohol consumption remained high in Ireland; the majority of Irish consumers exercised their sovereignty and, fortunately for them, no Irish equivalent of the Eighteenth Amendment was ever passed.

Temperance movements are indeed a fascinating subject of study, but their study should be supplemented by a deeper understanding of why people want to drink and why others are so keen on preventing them from doing so.

JOEL MOKYR  
Northwestern University

DERMOT KEOGH. *The Vatican, the Bishops, and Irish Politics, 1919–39*. New York: Cambridge University Press. 1986. Pp. xvi, 304. \$39.50.

JEFFREY PRAGER. *Building Democracy in Ireland: Political Order and Cultural Integration in a Newly Independent Nation*. New York: Cambridge University Press. 1986. Pp. xi, 259. \$34.50.

Dermot Keogh's study of the church and Irish politics during the period of the Anglo-Irish War and the Free State is the first major work to take advantage of the remarkable liberalization of access to ecclesiastical archives that has occurred in Ireland during the past decade. He has exploited a wide range of manuscript collections, both clerical and lay, especially the papers of John Hagan, rector of the Irish College in Rome (1920–30), whose importance may be slightly exaggerated because of the richness of his surviving correspondence.

Much of the book is diplomatic history in which the three "powers" are the Free State government, the Irish hierarchy, and the Vatican. Indeed because the archives of the Irish Department of Foreign Affairs are still generally closed, Keogh is giving us a rare look at the diplomats of the Free State. Students of Irish foreign affairs will be intrigued by the account of efforts to establish regular diplomatic relations with the Holy See, an enterprise of which the bishops were deeply suspicious. This quest on the part of some of the greenest players of the European diplomatic game included certain comic-opera features. Irish representatives in Rome in 1929, for example, were subjected to humiliation at the hands of Vatican professionals when they tried to fulfill a government minister's unfounded public prediction that a nuncio might make his state entry at the climax of the celebrations of the centenary of Catholic Emancipation, scheduled to take place in less than three weeks' time.

The availability of archival material clearly enhances the story pieced together by scholars who pioneered this field around 1970. Keogh is able to extend John Whyte's treatment of Eamon de Valera's writing of the constitution in 1937 and gives us an understanding of differences among churchmen over how far to go toward confessionalism. On the other hand, readers seeking to understand workaday postindependence church-state relations over such issues as divorce, censorship, and contraception will probably still turn first to Whyte.

The real value of Keogh's work is that it greatly improves our understanding of how the new state gained legitimacy. Ironically, the other book under review addresses this question more explicitly and with less success. Indeed, Jeffrey Prager's work might have been improved if the publisher had supplied him with a copy of Keogh's manuscript

and insisted that he rethink his argument in light of it.

Prager, working largely from secondary accounts, sets out to explain the achievement of political stability in Ireland between the formation of the Free State government under the terms of the Anglo-Irish Treaty in 1922 and the accession to office of an antitreaty government led by de Valera following the election of 1932. His explanation is framed in terms of political culture. Irish nationalist ideology, he argues, had two sides: an "Irish-Enlightenment" orientation, which envisaged a modern, secular, democratic order, and a "Gaelic-Romantic" emphasis, which harked back to a supposedly solidary native social order. He produces (p. 52) a table that cross-classifies four major leaders according to the cultural traditions from which each drew his norms and values. I find this a useful heuristic for understanding a complex political situation in 1922. These intellectual abstractions, however, will not bear the weight that the author tries to place on them in explaining the events of the subsequent decade.

The problem is not—as Prager seems to assume in a rationale addressed to his fellow political scientists—whether norms and values are the proper variables to investigate. Rather, he has excluded from his analysis the principal custodian of norms and values in Irish society, the Roman Catholic Church. From two rather defensive endnotes (notes 77, 78, pp. 235–6) I infer that this issue has already been pointed out to him. His answer seems to be that Whyte had characterized the church's role in state policy in this period as "unobtrusive." Keogh's work shows that apparent unobtrusiveness might well represent the determination of ecclesiastics to keep open their lines of communication with republicans who rejected the Free State. The Cosgrave government would have liked ecclesiastical endorsement in forms that would have equated the government with the state. Key churchmen understood better than statesmen the need to preserve the state as something more than an agency of the protreaty side and the church (Keogh, p. 134)—as more than "Cumann na nGaedheal at prayer."

DAVID W. MILLER  
Carnegie Mellon University

R. A. HOUSTON. *Scottish Literacy and the Scottish Identity: Illiteracy and Society in Scotland and Northern England, 1600–1800*. (Cambridge Studies in Population, Economy, and Society in Past Time, number 4.) New York: Cambridge University Press. 1985. Pp. x, 325. \$44.50.

"Used as a social indicator," writes R. A. Houston in his conclusion, "the study of literacy can provide



insights into some of the more general issues surrounding the comparative analysis of European societies, and in particular of *British* history as opposed to histories of England, Ireland, Scotland Wales" (p. 259). In this important revisionist work Houston presents perhaps the most successful monograph to date on the history of literacy. As the quotation suggests, the conceptualization of literacy and illiteracy is sophisticated and sensitive; in Houston's hands, it becomes a finely ground lens through which to examine a variety of different but nonetheless related topics and themes. In part, this book is a model of research and presentation. What is at least as significant, it is an essay in the sociology of historical knowledge and an exemplary account of literacy in historical context—from lived and recorded experiences to cultural mythologies and social theories, the kinds of expectations on which they both are so often based, and the sorts of policies they can engender.

Houston writes with several agendas firmly in hand. First, a revision of the "legend" and myth (terms he interchanges) of high, virtually unique, achievements in literacy and education in early modern Scotland galvanizes the work. He is not content with a "myth-or-image-versus-reality" conclusion but tackles the sources and the consequences, especially in terms of cultural politics, of Scotland's myths of literacy and education. Second, an explicitly comparative approach to literacy and its complex associations constitutes both his plan of research and analytic strategy. In expanding emphasis, Houston ranges from intra-Scottish comparisons (occupational, regional, communal, sexual, temporal, and so on) to Scottish-English, especially northern English (with which he argues for similarity rather than divergence) and international (European and North American). In so doing, he calls for a redefinition of British history and implicitly joins a growing international cry.

Third, whereas he carefully erects a quantitative skeleton of literacy's distribution and movements over time and place (using several different sources), Houston is sensitive to the limits of his evidence and excels in relating the testimony of an impressive range of nonquantitative information to the numerical inquiry. In so doing, Houston raises to a new level the study of the conceptualization of literacy—and its historical implementation—as a subject in its own right and as a means to examine a range of other important issues. Typically, many approaches, sources, and topics within the history of literacy stand in isolation from one another: distribution of statistical, signatory literacy, circulation and ownership of books, motivations for and uses of literacy skills, qualities and consequences of those skills, and so on. Houston interrelates them and illustrates methods for overcoming the dichotomies

and divisions (such as the literate versus the illiterate and the literary versus the oral) that so obstruct the field. In breadth of conceptualization and interpretation this book sets a new standard. Finally, by placing politics in its broad meanings central to the book and the interpretation of literacy, Houston makes a significant set of claims.

The book's range and results can only be suggested here. Worthy of further comment and constructive criticism are Houston's definitions and uses of comparative methods, the kinds of comparisons essayed, the relative imprecision of his arguments respecting the "reasons for literacy" and the relations between the oral and the literate, and the evidence used to undergird his general views about the hegemonic—following Antonio Gramsci—functions of Scotland's myth, ideology, and images of literacy. In most instances, I am highly sympathetic to his views; nonetheless, their discussion can only reflect positively on Houston's accomplishments and the future of this area of scholarly inquiry and interpretation.

HARVEY J. GRAFF  
University of Texas,  
Dallas

JEAN BOUTIER *et al.* *Un tour de France royal: Le voyage de Charles IX (1564–1566)*. (Collection historique.) Paris: Aubier, 1984. Pp. 409. 142 fr.

To see vividly the difference between social scientific and humanistic modes of research in our time, one need only spend some moments comparing the book under review with a work on the same topic that appeared five years earlier: Victor E. Graham's and W. McAllister Johnson's *The Royal Tour of France by Charles IX and Catherine de' Medici: Festivals and Entries, 1564–6* (1979). Graham and Johnson (Canadian professors, of French Literature and Art History respectively) glorify the royal tour by concentrating on literary evidence (editing, for example, the lengthy narrative written by a household official who accompanied the royal party) and by studying the pictorial representations related to the event and the personages involved. They retraced the itinerary personally to enhance their effort to revivify an extraordinary adventure of four centuries ago, when two thousand people traveled two thousand kilometers throughout France over two years' time.

The French authors of the new volume approach the event in a quite different spirit. Jean Boutier and his coauthors use every kind of evidence that is amenable to analysis to produce "une *anthropologie de politique*" (p. 8). Categories of analysis range from logistical problems of housing and feeding a veritable "cité entière" on the move to semiotic representations of the monarch under such rubrics

as "Les gestes et les signes." The value for the royal cult of the king's formal entry into a city—more than one hundred of them in all—is very well articulated. The middle section of the book will probably arouse the greatest general interest, for it represents the authors' goal of blending all the modes of analysis into a single political analysis, with its own laws and rhythm (p. 8). This end is served directly by charts and graphs correlating the duration of sojourns in different towns with meetings of the royal council and the issuance of edicts (by kind as well as number). The "state" in the sense of governing agency was ambulatory within the "state" as a physical entity. (The authors have a penchant for the voguish terms "espace" and "territoire.")

Only in recent years, after decades of relative neglect, have scholars of the Annales school begun to apply their rigorous social scientific methods to the study of monarchy. The work of Boutier, Alain Dewerpe, and Daniel Nordman is a worthy exemplar of this. But it by no means replaces the work of Graham and Johnson. The French scholars' work is more creative in that it widens the range of conceptual devices useful to understanding the impact of the prolonged and intense interaction between the king and his subjects during the royal tour. But the two Canadians' achievement—gained by the application of classical methods of philological and iconographical interpretation to the "archaeological" remains of the event—has autonomous worth in the realm of humanist scholarship.

RALPH E. GIESEY  
University of Iowa

PIERRE DARMON. *Damning the Innocent: A History of the Persecution of the Impotent in Pre-Revolutionary France*. Translated by PAUL KEEGAN. New York: Viking, 1985. Pp. 234. \$18.95.

Pierre Darmon's *Le tribunal de l'impuissance* (1979), translated as *Trial by Impotence* (1985) and reissued as *Damning the Innocent* (1986), is awkwardly translated and lacks footnotes and index. The book treats celebrated French trials held in the seventeenth and eighteenth centuries to decide the validity of marriage when a wife sought annulment on the grounds of sexual impotence. The main thesis invokes a convoluted theory of conspiracy that is full of contradictions, lacks historical context, and is overtly phallogocentric.

According to Darmon, the main villains of the conspiracy were voyeuristic church fathers and judges ("experts") and self-serving women (wives and their mothers) who sacrificed husbands on a "pagan altar" (the court) to the "myth of virility" (the marital requirement of conjugal relations). These ecclesiastical and lay judges, who possessed a "con-

fused and murky libido," or castration complex (pp. 1–3), transferred their own neuroses to hapless male victims, acquired control over sexual life at large, and expanded their sphere of social power through such jurisdiction. The sexually unsatisfied wives who brought such charges often concealed "sordid interests or . . . a desire for a change of partner" under the guise of piety and modesty, and they ruined families in the process (p. 103). Vengeful mothers and spiteful priests spurred them on, "for how else could the young woman presumed to be an innocent virgin explain the knowledge which made it possible for her to accuse her husband of impotence" (p. 103)? In these cases the legal system favored women, because the virulent hatred of impotent men superseded the misogynistic hatred of women in the Old Regime.

Although Darmon draws evidence from important legal commentaries and newsheets (listed in the bibliography) and from court briefs (identified only in the French edition), his conclusions rest precariously on a serious misreading of the court documents and the *mentalité* of the times. For instance, when women's suits for marital separation on ordinary grounds (cruelty, nonsupport, and so forth) were denied and women instigated new suits alleging impotence, Darmon assumes that the charges were fabricated. In fact, the documents reveal that the rigors suffered in trials alleging impotence were so dreadful that both parties were favored by the first route. Following the legists of the seventeenth and eighteenth centuries, Darmon views the "trial by *congrès*" (completion of sexual intercourse, confirmed afterward by physicians and midwives) as a scheme that backfired. Although devised by men to shame women, the *congrès* flourished, because women (increasingly shameless) knew they could win by such means. Yet the author does not account for husbands' continual insistence on the use of the *congrès* even after it was legally abolished in 1677. When judges ordered the legal sequestration of wives (tutorship in neutral space, such as parental homes and convents) for conjugal visits of accused husbands, Darmon labels the practice misogynistic. In fact, the documents show that sequestration saved wives from further physical abuse by frantic or angry husbands, protected wives against visitations by "surrogate husbands" clandestinely introduced to consummate marital acts (and perhaps engender pregnancies), and shielded wives from false accusations of adultery brought while the case was in progress. Finally, one may readily sympathize with the personal and public distress suffered by the afflicted men presented here. But in the context of early modern times, one cannot lose sight of the critical stakes faced by the women: the wish for a "normal" sexual and reproductive life, the legal right (with the birth of progeny) to share fully in



marital economic assets, and the right to freedom from sexual assault under the guise of conjugal duty.

SARAH HANLEY  
University of Iowa

WILDA C. ANDERSON. *Between the Library and the Laboratory: The Language of Chemistry in Eighteenth-Century France*. Baltimore: Johns Hopkins University Press. 1984. Pp. 190. \$22.50.

In this ambitious extension of the usual range of her discipline, Wilda C. Anderson applies the armamentum of literary criticism to selected texts from eighteenth-century chemistry. Although she distinguishes between science and imaginative literature, she correctly argues that characteristically Enlightenment concerns with epistemology and theories of language are embodied in such scientific works as Pierre Macquer's *Dictionnaire de Chymie* (1766) and the *Nomenclature chimique* (1787) of Antoine-Laurent Lavoisier *et al.* Literary analysis of these and related texts is thus especially appropriate and welcome.

The resulting intense, insightful readings seek to elucidate these texts' "sophisticated rhetorical and literary maneuvers" (p. 2). In general and particularly with Macquer's *Dictionnaire* and Lavoisier's early papers on water, Anderson achieves this end admirably. But the reader unfamiliar with the language of modern criticism, put off by a barrage of analytical expressions such as "semantic fields," "emplotting procedures," and "slippage points," or unfamiliar with the "classical *épistémè*" discussed elsewhere by Michel Foucault will likely find this work opaque.

Anderson pushes critical theory to its limits and makes several strong claims about the history of chemistry and the nature of scientific change that would seem more than her analytical apparatus should bear. For example, she would convince us of the "generation of scientific knowledge [solely] as a result of discursive procedures" (p. 3). She likewise offers a radical reinterpretation of Lavoisier, which sees his "new valorization of method" as his fundamental contribution to chemistry (p. 137). Lavoisier's method is not his positivism or the historiographically familiar "chemistry of the balance sheet"; rather, citing the critic Jean Starobinski, Anderson sees it as a transcendent approach to producing new knowledge, one for which "no stable definition" exists (p. 131). Then she suggests that with Lavoisier one first observes "the institutionalization of science as its own speaking voice" (p. 151). This "voice" seemingly represents a rhetorical stance of science as anonymous and autonomous once expert scientists agree on a common language for research.

The discipline Anderson represents (and her talent) allow her many sophisticated points; the notion that scientific "facts" are theory-laden is certainly one. Each deserves separate consideration, but in general the very limitations of her inquiry seem to undermine her major arguments. Despite a broad definition of "discourse," she considers only textual evidence. She largely passes over technical chemistry and laboratory procedures, and someone unaware of both may get lost. She makes no appeal to historical evidence to answer such key questions as whether, even though Macquer may have been a lone chemical *philosophe*, the community of phlogiston chemists acted other than united at the research front. Finally, little use is made of relevant vocabularies from the philosophy of science. In particular, Anderson essentially ignores the work of Thomas Kuhn, which is odd because she takes care to cite Ludwik Fleck and her own extensions of his work (pp. 63, 141). Sustained considerations of the philosophical literature would have enriched her views of the roles of authorship, textbooks, and creativity in science.

JAMES E. MCCLELLAN III  
Stevens Institute of Technology

ROBERT M. ISHERWOOD. *Farce and Fantasy: Popular Entertainment in Eighteenth-Century Paris*. New York: Oxford University Press. 1986. Pp. ix, 324. \$29.95.

Two competing approaches to popular culture inform histories of eighteenth-century France. One sees culture as a set of shifting responses to great events and social transformations, including the growth of the Enlightenment. The other treats it as an autonomous system with many features that change slowly, if at all. Robert M. Isherwood adopts the second approach, rejecting interpretations of popular culture as a product of class divisions or of political change. His rich, pleasurable evocation of popular entertainment in Paris from roughly 1700 to 1780 concentrates on a series of concrete cases, arranged in loose chronological order: Pont-Neuf; Parisian fairs; the Opéra Comique; the struggle between *forains*, the Opéra Comique, and licensed theaters; the emergence of boulevard theaters; the Palais-Royal. Twenty-odd illustrations, consistently interesting if muddily reproduced, complement the abundant citation of texts and incidents. (The illustrations do not, alas, include a map of eighteenth-century Paris on which one could locate the many streets and buildings mentioned in the text.)

Although the book makes passing references to cafés, dogfights, public baths, prostitution, and other diversions, it stresses the theater and related public spectacles. Through anecdotes and pictures we learn of Big Thomas, the Pont-Neuf's gregarious

tooth-puller; Fanchon Chemin, the lovely street-singer who inspired so much popular literature; the terrible burning of the Saint-Germain fair in 1762; the more than one hundred people killed in a panic at the fireworks show in the Place Louis XV in 1770; and many more colorful people and adventures. But the book goes beyond casual cultural history; Isherwood's intensive examination of eighteenth-century materials, for example, includes the systematic analysis of thirty-eight theater pieces produced by Jean-Baptiste Nicolet, founder of the Grands Danseurs of the Boulevard du Temple. The text overflows with verses, plot summaries, and comments of contemporary observers. (Since Isherwood quotes many French texts in the savory original but fails to translate them, readers with limited knowledge of the language will miss a significant part of the message.)

If the book proceeds by means of detailed descriptions, it eventually develops a story and an argument. The story traces the emergence of new sites of entertainment, passing through the displacement of fairs by the boulevards and culminating in the crowded world of the Palais-Royal. The argument denies the separation of popular from elite culture in eighteenth-century Paris, at least in the domain of entertainment on the street and in the theater. Isherwood argues, indeed, that the world of entertainment provided a welcome point of contact between classes of people that the routines of work and formal sociability separated. He also shows that the creation of royally sanctioned monopolies of some kinds of entertainment, such as the spoken plays of the Comédie Française, shaped the evasions of those monopolies in the forms of mime, vaudeville, and other popular pleasures. The two arguments connect in the idea that royal protectionism drove a wedge between the elites and the masses: "Thus," says Isherwood, "separation between popular and elite theatrical entertainment was rooted in the politics of privilege" (p. 97). As the century wore on, he concludes, elite and popular theatrical cultures again converged, as aristocratic patrons such as the duc de Chartres sought and promoted mass entertainment. The apparent success of the *Opéra Comique*, which moved away from earthy slapstick to moralizing romances, raises some doubt about Isherwood's thesis; the uncertainty of the evidence concerning the audiences for different sorts of shows, furthermore, leaves a crucial part unproved. In any case, the author's pleasure—and ours in reading his book—lies less in the pursuit of a thesis than in reconstructing the teeming spectacle of eighteenth-century Parisian popular life.

CHARLES TILLY  
*New School for Social Research*

TIMOTHY TACKETT. *Religion, Revolution, and Regional Culture in Eighteenth-Century France: The Ecclesiastical Oath of 1791*. Princeton: Princeton University Press. 1986. Pp. xxi, 425. \$45.00.

The title and subtitle of this important book might well be transposed, for Timothy Tackett is less concerned with generalities than with a specific historical problem. He seeks to explain the reaction of French priests to the Ecclesiastical Oath demanded of them by the revolutionary National Assembly in 1791, an oath that ultimately implied acceptance of the proposition that priests were primarily public servants.

Searching statistical analysis enables Tackett to concentrate on the predominance of jurant or nonjurant priests in different geographical areas. He then tries to correlate this pattern of distribution with others indicative of the characteristics of the clergy, such as the recruitment, training, and political experience of priests. Although previous participation in collective activity here appears important, no single factor emerges as significant throughout France. Tackett therefore extends his inquiry to include the characteristics of the communities in which the priests served, a step justified by the argument that few priests could have been entirely unaffected by their parishioners' opinion, even though many may have chosen to defy it. This much broader examination of social situations is also revealing: we see, for example, that, in places where Protestantism was strong, priests generally tended to regard the revolution as a further threat to Catholicism and so to reject the oath. Overall, however, the inquiry again fails to expose any single explanation of reactions to the oath that is valid for the whole of France. Tackett can only allow the probability that different areas were affected by differing combinations of some particularly significant factors, so that the priests in them were predisposed to accept or reject a primarily social role. Ultimately, therefore, he sees the crisis of the oath as not just a precipitant but "a seminal event in its own right" (p. 299).

This book, the record of an inquiry rather than the exposition of a thesis, gives new precision to some recognized features of the field and places these and others, far less familiar, in fresh perspectives. To be sure (a phrase, incidentally, that is used too often in these pages), Tackett is not much concerned with theological or philosophical abstractions. Indeed, even the importance of national sovereignty is only occasionally acknowledged. Yet, since he is singularly sensitive to shades of distinction, his work is magisterial in judgment and fascinating in detail. Here, as in his earlier work on the Dauphiné, Tackett writes excellent social history, combining quantification with collective biography

and illuminating all by frequent references to the lives of individuals. Nowhere can the reader forget that the priests' dilemma involved many of them in great spiritual anguish.

Since Tackett often notes that research in some areas is not sufficiently advanced to permit estimates of probability, to describe his book as definitive may be premature. But, as a sophisticated and comprehensive assessment of a vital question and as a book richly informative about French society in 1791, it will be indispensable to all students of the revolution.

MICHAEL J. SYDENHAM  
Carleton University  
Ottawa, Canada

GUY CHAUSSINAND-NOGARET. *Madame Roland: Une femme en révolution*. Paris: Seuil. 1985. Pp. 358. 89 fr.

This is a book with an old-fashioned view of "History," written with a capital *H*. It opens with neither ruminations on discourse nor a new theory about the origins of revolution derived from statistical evidence from the provinces; instead it privileges the melodrama of events, crises, and the main characters of the French revolution centered in Paris. The heroine, Marie-Jeanne (Manon) Phlipon Roland, wife of the Girondin minister of the interior Roland de la Platière, is presented with éclat as "a woman possessed by History," "the luciferian female of Jacobin demagoguery," and as being, with Danton, "the most fascinating of the revolutionary actors" (p. 10). Paradoxically, this is also a book of "History" envisioned as an erotic act. The author portrays himself as a lover of the past, whose ambition is to re-create its characters for present-day readers. Such a florid introduction—and what follows—cannot help piquing the reader's imagination. This is a book aimed at what the French call *la grande publique*, the general public. This is history as Gothic novel.

Guy Chaussinand-Nogaret's book is well informed, although it lacks the detailed notes that scholars value and that are available in earlier biographies, including Gita May's excellent *Madame Roland and the Age of Revolution* (1970). If the book is semifictionalized in places and offers little new documentation of the short and amply documented life of Manon Phlipon Roland, it certainly qualifies—on the surface—as a "good read."

Some reservations are in order, however, about the author's interpretation of this famous woman's life and her relationship to the French revolution. This book sadly lacks the insight that might have been gained from the new scholarship in women's history and the ensuing possibilities for reinterpreting the extant sources. Readers are introduced to

Manon Phlipon as a young adult woman, living with her recently widowed father. Throughout the book Chaussinand-Nogaret privileges his subject's relationships with men to the exclusion of Manon Phlipon Roland's own valued female friends and associates, including her mother. Manon Roland's longtime friends, Sophie Cannet and Henriette Cannet, whom she met during her year in a convent school and corresponded with in great detail over many years, remain only shadowy presences in this study. Similarly, Manon Roland's distinctive characteristics—her singular upbringing as a gifted, largely self-educated, and highly cultured girl, given to books and intellectual conversation and to the elaboration of a fully articulated approach to marriage and motherhood, and her conscious veto of a number of suitors prior to marriage in her midtwenties—are, for this author, "on the fringe of history." They are disposed of in the first seventy-five pages of the book. Her girlhood itself has, it seems, barely any "historic" significance. For this author, in contrast to his colleagues at the *Ecole des hautes études*, the history of private life is not history at all. Historians of women will strongly disagree.

The book concentrates on the revolutionary years 1789–93. In the author's view, only with the revolution is the heroine, then in her midthirties, "captured by History, its consenting prey." The remainder of the book deals with the four years that ended with Manon Roland's execution by the Jacobins in 1793 for political crimes.

Ironically, Manon Roland, who decried the mixing of women into political life and tried consciously to stay out of the limelight, was accused and condemned by the Jacobins for doing exactly the opposite. It is also ironic that she subsequently became, through her published memoirs and correspondence, one of the best-known women of late eighteenth-century France. Chaussinand-Nogaret dwells on her affair of the heart with Buzot, her antipathy for Danton, and her initial admiration for Robespierre. But he sidesteps the thorny questions that can be posed concerning both the character of the well-documented political presence of women in the revolution and the gendered content of revolutionary acts. The new historians of women will doubtless make more insightful—though perhaps less flamboyant—use of this material in reinterpreting Madame Roland with reference to the French revolution.

KAREN OFFEN  
Stanford University

ROBERT LOUIS STEIN. *Léger Félicité Sonthonax: The Lost Sentinel of the Republic*. Cranberry, N.J.: Associated

University Presses, for Fairleigh Dickinson University Press, Rutherford, N.J. 1985. Pp. 234. \$29.50.

Official representatives sent on missions by the governments of the French revolution often took important initiatives within their broad powers. One of them is the subject of this book: Léger Félicité Sonthonax, who was sent to St. Domingue, first as delegate of the Convention (1792–94), and later as agent of the Directory (1796–97). In August 1793 Sonthonax abolished slavery in northern St. Domingue, six months before the Convention issued the official decree of abolition. In 1789 Sonthonax was twenty-three; at fifty, he died under the empire: the French revolution frames his adult life. Robert Louis Stein's book, the first on this interesting figure, brings out his crucial contribution to the history of St. Domingue: "To dare proclaim—as he would himself recall—the Rights of Man in the new world" (p. 181).

After tracing Sonthonax's family origins and formative years, part 1 describes his political beginnings in the 1792 Jacobin Club dominated by the Gironde. Parts 2 and 3, the core of the book, deal with the two successive missions to St. Domingue. Part 4 describes the last years of a Jacobin, constantly watched by Napoleon's police because he remained faithful to his radical principles.

The book is useful primarily as a study of the impact of the French revolution on St. Domingue, rather than of the internal dynamics of the revolution in St. Domingue-Haiti. It sheds new light on the emissaries sent overseas by the new republic and on the nature of its colonial policy, which their actions defined. As a biography, it has certain limitations, because of the sources available. In the absence of private papers, Sonthonax's personality remains elusive, and, with the dependence on the subject's official writings, the study at times reads like an apologia.

What was most commonly known of Sonthonax, before Stein's book, had been drawn from the vitriolic literature of his opponents: St. Domingue *petits blancs* that resented his egalitarian policy, later black and mulatto leaders that feared his rival—revolutionary—power, and the broad anti-Jacobin reaction that grew after 1794 in France itself. From Stein's firsthand research emerges a strong but not "monstrous" figure, with a paradoxical trajectory resembling that of many other Jacobins. The son of a well-off merchant family, Sonthonax showed a concern for social justice; raised far from the Atlantic and colonial France, he developed expertise on the colonial question and became an abolitionist.

"Vive la République, vive la liberté" shouted thousands of "new citizens" when Sonthonax proclaimed the abolition of slavery (p. 89). Here were the principles that underlay the colonial policy of the

French revolution: all individuals were now personally free, but St. Domingue remained a colony. All the regulations and measures he issued reflected his vision of a colonial society regenerated by the French revolution, where the plantation economy was revitalized by a free labor force. Stein shows how the war issue undermined the implementation of this policy and how military leaders, among whom more and more were native, came to supersede civilian officials such as Sonthonax. Toussaint-Louverture (backed by French general Etienne Laveaux) was responsible for abruptly ending Sonthonax's second mission and sending him back to France. The administrative frame of Sonthonax's policy might have been better treated comparatively, for it falls by and large within the general lines adopted by all revolutionary appointees overseas. But, equally important, Stein argues that Sonthonax's policy served as a blueprint for the first Haitian leaders, who dreamed of a free nation of disciplined workers and an economic prosperity resting on large state-owned plantations.

ANNE PÉROTIN-DUMON  
Kent State University

MARCO FERRARI. *Frédéric Lullin de Châteaueux: Trasformazione della società e circolazione delle aristocrazie*. (Quaderni dell'Istituto di Scienza Politica, Università di Genova, Pensiero Politico, number 3.) Genoa: E.C.I.G. 1985. Pp. 152.

Frédéric Lullin de Châteaueux is a much neglected figure, not least because he insisted on publishing his reflections on political, social, and economic matters anonymously. In his own lifetime he was known (beyond the narrow circle of his friends and close associates) as an expert on agricultural technology and management. Yet he was deeply interested in the wider issues that occupied liberal intellectuals in France, Switzerland, and Italy in the years following the fall of Napoleon. He was in fact an active member of the group that met at Coppet under the aegis of Madame de Staël. And, indeed, the best known of his anonymous writings, the *Manuscrit venu de Sainte-Hélène d'une manière inconnue*, was once presumed to be the work of either Madame de Staël or Benjamin Constant. His work is significant, if not richly original. Marco Ferrari's study will go some way toward establishing his rightful place in the history of ideas.

The work under review is divided into two sections. In the first Ferrari gives us an outline of an intellectual biography. He deftly handles the efforts of the liberal aristocracy to come to terms with the aftermath of the French revolution and Napoleon. Châteaueux's liberal elitism was finely balanced between the traditionalism of the *ancien régime* and

the wider civic idealism that had been discredited (at least in aristocratic circles) by the French revolution. Ferrari emphasizes the political realism that sustained the liberalism of the Coppet circle, designed to secure the principal ideals of the Enlightenment rather than simply to defend the status quo. Châteaueux's ideas are explained in relation to those of his circle (in particular, Madame de Staël, Constant, Jean-Charles-Léonard de Simonde de Sismondi, and the young Cavour). And, indeed, he can be said to have exercised an influence on the later development of liberalism largely through them.

The second part of the book is a closer piece of analysis. Ferrari examines the leading themes of Châteaueux's *Lettres de Saint-James*, his most important statement of political ideas. What emerges (above all) is the need for liberal theory to take account of the impact of changing modes of economic organization on the relations between groups and classes. In Châteaueux's view, a flourishing liberal polity would emerge if a balance of interests were sustained within a society. This was the key to the success and stability of England in the eighteenth century. French theorists, however, had seen the balancing of opinions as their principal concern, with political instability the inevitable consequence.

In Ferrari's account, the roots of Châteaueux's liberalism are to be sought in political economy rather than political theory. Anyone interested in the changing preoccupations of liberal thinkers in the 1820s will find this brief study rewarding. What we still need, however, is a full-scale intellectual biography.

B. A. HADDOCK  
University College of Swansea

BRIAN WILLIAM HEAD, *Ideology and Social Science: Destutt de Tracy and French Liberalism*. (Archives internationales d'histoire des idées, number 112.) Dordrecht: Martinus Nijhoff; distributed by Kluwer, Hingham, Mass. 1985. Pp. vi, 229.

In the past dozen years a number of first-rate studies have appeared on Destutt de Tracy and the French *idéologues*. Emmet Kennedy has written a subtle, richly documented intellectual biography of Tracy. Sergio Moravia and Georges Gusdorf have produced erudite reassessments of the *idéologues'* movement. And most recently Cheryl Welch has published an elegant and lucid study of the *idéologues'* reformulation of utilitarian and liberal ideas in the aftermath of the French revolution. Although Brian William Head's new book lacks the distinction of these recent works, it offers the first reliable and relatively comprehensive survey of Tracy's thought.

This book is a thorough and careful, if rather pedestrian, survey of Tracy's philosophy and political thought. The author gives a minimum of biographical and contextual information. His main concern is to show the interrelation of Tracy's ideas and to specify inconsistencies and unresolved problems. Particular attention is given to three topics. The first is Tracy's conception of *idéologie*, the "sensationalist" science of ideas through which he hoped to establish incontestable truths about the individual's capacities and needs. The second is Tracy's conception of *la science sociale*, his attempt to elaborate a science of social organization on the basis of these truths. The third topic is Tracy's liberalism—his attempt to find some balance between the principles of democracy and enlightened leadership in his proposals for political, economic, and educational reform.

The chief quality of this book is the author's meticulous care in the restatement and critical analysis of Tracy's positions and arguments. Its main weakness is Head's reluctance to generalize or develop his critical insights or to establish an interpretive perspective. The result is a largely descriptive work that offers more commentary than argument. The final chapter includes some interesting reflections on the roots and implications of Tracy's conception of scientific certainty. But all we are offered in conclusion is the bland assertion that Tracy was an heir of the Enlightenment and the more provocative but largely unsubstantiated claim that "one of the most striking aspects" of his work was "his use of science as a type of all-encompassing explanatory system, amounting to a form of secular faith" (p. 208).

Faith or science, Tracy's ideas have now lost most of their resonance. Head himself often notes Tracy's dogmatic scientism, his reductionism, and the narrowness of his view of human nature. For that very reason, however, and also because Tracy was a fundamentally derivative thinker, one wonders whether his thought is best studied as an isolated philosophical system. The more contextual approaches of Welch and Kennedy seem better to communicate a sense of what made Tracy's ideas so compelling to contemporaries as different as Thomas Jefferson and Stendhal.

JONATHAN BEECHER  
University of California,  
Santa Cruz

DAVID H. PINKNEY, *Decisive Years in France, 1840–47*. Princeton: Princeton University Press. 1986. Pp. xii, 235. \$30.00.

One of the most respected French historians, David H. Pinkney, has presented us an important book



that defines the years 1840–47 as a watershed in modern French history. Pinkney begins by noting—and it will be debated—that the *ancien régime* was still basically in place in the mid-1830s. France was predominantly rural, disunited, dominated by *grands notables*, and in many ways backward. The democratic innovations of the revolutionary period were “alien grafts in a hierarchical society” (p. 4).

The 1840s brought fundamental economic, social, and cultural changes that influenced politics as well. Something of an economic determinist, Pinkney places special emphasis on an industrial take-off, spurred by the iron and railway revolutions. The results were innovations in banking, a boom in Parisian population, national marketing strategies, and the first significant mitigation of provincial isolation. The introduction of the telegraph and an efficient postal system (the postage stamp dates from 1849) played a role as well. Together with changes in education wrought by the Guizot law—well explained here—these developments contributed to the consolidation of France described in the third chapter, “Centralization Made Real.” Pinkney’s next chapter, “Search for New Identities,” handily establishes a collective identity crisis in France and how it was somewhat overcome, particularly through the institution of corporate identities in a variety of professions, including the new working class. One learns of the formation of a society for architects, one for engineers, and, under the aegis of Balzac, a mutual aid society even for that stubbornly individualistic species, writers. Teachers too became more professional, although “the indolent, the incompetent, and the misfits who had previously drifted into teaching” may not all have disappeared, as Pinkney implies (p. 82).

Pinkney then dips into the cauldron of new ideologies, especially positivism and socialism, painting Paris of the 1840s as an exciting laboratory of the future. He effectively cites Alexander Herzen: “I entered [Paris] with reverence as one used to enter Jerusalem and Rome” (p. 99). In a subsequent chapter he moves to artistic changes wrought by realists such as Courbet and Flaubert and “blasphemers” such as Baudelaire, as well as other innovations, such as democratic libraries and newspaper serials. Architectural strides using the new iron are noted, although the relevance of Eugène-Emmanuel Viollet-le-Duc’s medieval restorations to Pinkney’s thesis is perhaps questionable.

Finally, Pinkney notes a shift in French foreign policy during the 1840s, marked by the healing of traditional Anglo-French rifts and the beginning of colonial settlement in Algeria. A short conclusion summarizes these changes and points to a “new, less inhibited generation,” liberated from the past and ready to go forward.

Although I deplore ideological wrangling, Pinkney might have engaged other historians of French modernization (Eugen Weber, Charles Tilly *et al.*) throughout, not just at the beginning. One would also like to know how important changes felt to people. An example would be Daguerre’s photography, which Pinkney deals with well, but too curtly. If this was indeed a dawn, more would be welcome on the bliss or other emotions it inspired.

Pinkney’s forte here is economic history: he truly grasps how things got done and at what cost. In cultural history he is a little too mindful of secondary authorities. Mentioning the success of a serial such as Eugène Sue’s *Mystères de Paris*, he might have cited passages to give a better feel for a book little-read today. Some will also question his idea that Flaubert was fully formed by 1845 or wonder why the titles of Marx’s early writings are given in German.

But how much one learns here! And what useful, clear distillations—for example, on medical personnel, primary education, and especially the geography of Paris, which Pinkney knows so well. At times the book seems a trifle scrupulous, but the thesis is challenging, the prose and facts presented will allow this essay-monograph to cross markets (from scholar to undergraduate to lay person), and the author’s dignity and seriousness are apparent on each page. Pinkney’s new book extends, complements, and to an extent summarizes an *oeuvre* that has always bespoken the true professional at work.

BARNETT SINGER  
Victoria, B.C.

PHILIP G. NORD. *Paris Shopkeepers and the Politics of Resentment*. Princeton: Princeton University Press. 1986. Pp. xvii, 539. \$47.00.

The problem of the lower middle class is central to the history of late nineteenth- and twentieth-century Europe. Generalizations about the issue abound, but, for France at least, empirical scholarship is slender. In an important book Philip G. Nord makes a major contribution to our understanding of the problem: a detailed study of the modest but not marginal retail businessmen in the clothing and luxury trades of inner-city Paris, the *petits commerçants*. The beginnings of retailer protest go back to the late 1880s and the formation of the Ligue syndicale. The immediate object of shopkeeper’s self-mobilization was the department stores, whose high turnover and small markup appeared to threaten small-scale commerce. As Nord notes, the *grands magasins* were not the real cause of the shopkeepers’ economic difficulty. They were, however, the symbol of more fundamental changes in the economic structure of Paris. The most notable of



these was what Nord calls Haussmannization, with its attendant transformation of the commercial geography of Paris, turning once vibrant commercial districts such as the Palais Royal into stagnant backwaters. The economic crisis of the 1880s led in turn to the famous *révolution du bon marché*, the proliferation of cheap, mass-produced consumer goods. The "democratization of luxury" profited lower-class consumers and large-scale retailers; it spelled ruin for the small businessmen. The resultant resentment of shopkeepers found an echo in other strata of Parisian society, horrified by the egalitarianism of the new boulevards, the substitution of cheap bouillons for the elegant *Café d'Anglais* and of theaters for the proliferating café-concerts. In fact, the subject matter of this book is, for once, wider than the title suggests, and the reader is treated to some delightful sections on the world of the boulevardier.

What form did the politics of resentment take? Nord challenges the assumption that the discontent of small owners must automatically be reactionary. True, in economic matters, militant shopkeepers were consciously antimodern and clung to an idealized version of the past. Yet in the 1880s they also identified with the Radical republicans and shared their demands for direct democracy, the income tax, minimum-wage legislation, and municipal socialism. They were Jacobins rather than liberals, perhaps, but very much on the Left nonetheless. Even the less-savory aspects of their protest, anti-Semitism and xenophobia, for example, did not distance them very much from the Jacobin tradition. The ease with which many small retailers embraced Boulangism testifies to the perceived Jacobinism of Parisian Boulangism rather than to the reactionary reflexes of the *petits commerçants*. Still, the radical Left was in a state of flux by the last decade of the century, and the Jacobin tradition was about to be overtaken by a more "modern" socialist one. In the 1890s shopkeepers drifted to the Right and supported Christian Democracy, anti-Semitism, and nationalism. But, Nord contends, this evolution was not inevitable. Shopkeepers ended up on the radical Right because avenues of redress on the Left were closed to them. The militant collectivism of the socialists and their support for consumer republicanism, catering to the interests of large-scale commerce, offered even less. Nord suggests that the Left abandoned the independent retailer, not the other way around. The book ends with a subtle discussion of the relationship of class and politics and a compelling argument against mechanistic sociological reductionism.

This carefully crafted and elegantly written book should be read by all students of society and politics in late nineteenth-century France. It is also a won-

derfully enjoyable book for anyone who has ever walked the streets of old Paris.

WILLIAM D. IRVINE  
York University

LOUIS PÉROUAS. *Refus d'une religion, religion d'un refus en Limousin rural 1880-1940*. (Recherches d'histoire et de sciences sociales, number 12.) Paris: Ecole des Hautes Etudes en Sciences Sociales. 1985. Pp. 245. 120 fr.

In the tradition of the sociologists of religion Gabriel Le Bras and Fernand Bouhard, Louis Pérouas considers the marked detachment of the people of the rural Limousin from the Catholic church, in the years 1880-1940. He supplements a wide variety of archival sources (including the archives of several religious congregations and of the Grand-Orient of France) with a number of interviews and a survey of the number of tombs with crosses in several cemeteries. The result is a book of both merit and interest.

Although acknowledging the "pertinence" of the term "de-Christianization," Pérouas pointedly avoids its use. Carefully distinguishing between "the detachment from Catholicism and changes in certain of its expressions" (p. 200), he concludes that, in the Limousin, "the population was detached less directly from religion and even from Christianity than from the clergy" (pp. 201-02). This assumption leads him to offer a caution against reading too much into the indices of religious practice that he has himself used so successfully, such as the delays in baptism (not easy work—one local historian examined over one hundred fifty thousand baptismal acts in the Creuse), percentages of civil marriages and burials, fulfillment of the church's Easter obligation, and attendance at Sunday mass.

The detachment of ordinary people, particularly men, from organized religion in the Limousin, evident early in the nineteenth century, was greatly accentuated during the first decade of the twentieth century. In the canton of Eymoutiers, which became a Communist stronghold in the Haute-Vienne, only 1.3 percent of the men went to mass on Sunday, and 2.1 percent fulfilled their Easter obligation, compared to 17.2 percent and 14.2 percent of the women, respectively, in 1922-25. In the region of Saint-Sulpice-les-Champs, arguably the regional grand champion of de-Christianization, the percentage of newborns not baptized leapt from 8.7 percent in 1897-1904 to 45.2 percent during the 1906-13 period; around Le Grand-Bourg, from 5 percent to 64 percent in 1906-13. This may suggest that the formal separation of church from state in 1905 accentuated the detachment from organized religion. In one canton in the Creuse not a single

church burial took place in 1906. Pérouas explores more long-term causal factors (and the clergy's response), including the influence of the *franc-maçons* and freethinkers, as well as the dominance of the political Left in the Limousin.

Pérouas assesses the quality of the clergy (denounced by its enemies as money-mad and sometimes immoral). Reliable evidence is sketchy: the subprefects of the Creuse believed that twenty-seven of one hundred thirty-one priests were of dubious morality, including one "peu scrupuleux en affaires," one who drank too much, three hypocrites, two gamblers, and ten suspected of "relations sexuelles." It is difficult to know how much of this was true, of course; the bishop of Limoges reportedly sent his least able priests to the Creuse and saved his best people for more promising regions.

The chapter "Anticléricalisme et besoins religieux" argues that the rituals and rhetoric of freethinkers and other anticlericals represented something of substitute religion—the extension of the argument that the people of the Limousin left the clergy but not necessarily the church. One man, in a canton with many freethinkers, told a priest, "You speak of the good God and of the Holy Virgin, they say nothing! You speak of priests, they see red" (p. 192)! In 1908 the socialist freethinker Emile Noël declared "we need a religion of the love of man for man" (p. 164), which Pérouas calls a "religion of refusal." In any case, capitalizing on the unpopularity of the clergy and its identification with political reaction, anticlericalism won the day. How many Limousins who were not *pratiquants* were still *croyants* cannot be determined, even by such an able historian as Pérouas.

JOHN MERRIMAN  
Yale University

PATRICIA HILDEN. *Working Women and Socialist Politics in France, 1880–1914: A Regional Study*. New York: Clarendon Press of Oxford University Press. 1986. Pp. viii, 307. \$34.40.

This study of female textile workers in Lille, Roubaix, and Tourcoing and of their relations with socialist political parties and trade unions during the *belle époque* is described, in the subtitle, as a "regional study," but the significance of Patricia Hilden's findings reach far beyond the Nord. I recommend it not only to French historians of this period but also to historians of leftist politics, of the working class, and of women in countries other than France. Factors that are advanced to explain working women's lesser participation in unions and socialist politics than men's—especially male leaders' unwillingness to recruit female workers and the men's inattention to women's particular concerns—operated in other

countries as well, but Hilden's closely detailed study of three cities over a period of almost forty years demonstrates that the history of female workers and socialist politics is not uniform or unchanging. Although early in the *belle époque* female workers in the Nord were actively involved in leftist politics, their participation subsequently diminished—but never completely disappeared—and their once greater involvement was quickly forgotten. Hilden's detailed study of one region allows us to see the causes of this change and even to identify ways that this history might have developed differently.

The story divides into two periods. The first, from the late 1870s to 1897, is one of active grass-roots organizing among female textile workers. Jules Guesdes's Parti ouvrier addressed women as workers and included them and their demands for equality in party activities. Women created separate groups in which their special interests could be most comfortably discussed. National women leaders spoke in these towns and drew large crowds. Local women leaders emerged. Women's presence in strikes, demonstrations, and meetings was recognized and appreciated.

Beginning in the last years of the century, however, socialist union leaders, in their desire to strengthen the federation of textile unions, dropped what they had come to perceive as an unpopular position favoring women's right to work as equals to men and instead proposed raising men's wages so that women could "return" to the care of the home. Socialist party leaders similarly changed their once progressive attitudes toward women. Once they switched their strategy to winning elections, concerns for the rights or independence of women—who, after all, could not vote—ceased to matter to them.

Hilden's history of this change is carefully nuanced. Reactionary and progressive ideas coexisted even in the earlier period. Women, more scrutinized by employers, more busy, and more impoverished than male workers, were always fewer in numbers and lesser in power in these movements than men. On the other hand, women's active participation in the rich socialist culture of these working-class communities survived into the later period despite the neglect or outright hostility that characterized the organized movement. Hilden sets this history against the background of the culture of French Flanders—a culture characterized by less sex segregation in both workplace and social life than prevailed elsewhere in France (or in most other countries at this time). The descriptions of how workers lived, worked, demonstrated, and amused themselves are wonderfully alive.

Hilden's conclusions are interesting and challenging. On occasion, however, she fails to identify historians she challenges, writing instead of "many

historians," "recent historiography," or "received wisdom." Readers may wonder, therefore, as I sometimes did, if she attacks only "straw men." For example, her claim that "women saw their relation to production and reproduction in a more complex way than much recent historiography has suggested" (p. 275) ignores recent histories of working women that have argued just this. Further, "the view that marriage freed women from waged labour" (p. 35, also pp. 278–79) is not current among historians of French women. These points do not, however, detract from my overall appreciation of this well-researched, well-argued study and for its convincing evidence for feminism's strong influence outside of Paris and among the working class. Unfortunately, nineteenth-century socialists failed to overcome the theoretical problem of blending Marxism and feminism. Hilden, however, has done so very successfully.

CLAIRE G. MOSES  
University of Maryland

TONY JUDT. *Marxism and the French Left: Studies in Labour and Politics in France, 1830–1981*. New York: Clarendon Press of the Oxford University Press. 1986. Pp. ix, 338. \$29.95.

After two monographs on particular aspects of nineteenth- and twentieth-century French socialism, Tony Judt turns to a broad survey of this most studied of French political traditions. The result is stimulating but not easily categorized: it is neither a comprehensive survey of Marxism and the French Left, as promised by the title, nor a set of disconnected essays, as the subtitle threatens. The author devotes close attention to only five topics, some broad, others rather narrow, but in the process he conveys a coherent view of French socialist history. This work deserves to reach a wide audience—and reaches out half-way toward one—but perhaps requires too much prior knowledge to become popular. Specialists will find some received ideas challenged—and, indeed, Judt has some harsh words for much socialist historiography—but he demonstrates the benefits of studying Marxism from the "outside."

The strength of Judt's interpretation comes from his understanding that French socialism belongs "to the revolutionary tradition in a land where politics always took precedence over economics in determining collective behaviour" (p. 21). Precisely for this reason, he argues, Marxism was readily assimilated into the mainstream of French socialism in the 1880s and 1890s; Marx's analysis of the French political situation was sound even though the French case showed his theory of history to be nonsense. After demonstrating the futility of a

Marxist explanation of the history of the French labor movement, Judt issues a welcome call for "a demythologized history of French labour" (p. 113).

Judt's examination of the Section Française de l'Internationale Ouvrière in the 1920s and 1930s also argues the failure of any Marxist interpretation, for he shows a party with a doctrine and a strategy designed to serve the interests of the proletariat yet largely at variance with "the sociological configuration of either its membership or its voters" (p. 158). Yet, the claim of the socialists who supported Jean Jaurès and Léon Blum to be Marxists was as legitimate as the claim of the post-Tours Communists. Judt also suggests—heretically, but with good reason—that Blum's political achievements were in many respects more remarkable than those of his mentor, Jaurès.

Judt is also critical of the historiography that tries to separate the leftist intellectual tradition in France from the popular socialist and labor movements. His post-mortem on the flourishing of the Marxism of the French intelligentsia after 1945 is devastating ("Marxism became the subject matter of marxists, where once that role had been filled by the real world of social relations" [p. 174]), although it reveals a certain "Anglo-Saxon" prejudice toward the French culture of the Word. Although he argues that the intellectual passing of Marxism is not to be regretted, Judt's work ends—in his historian's look at the election of François Mitterrand to the presidency—on a note of loss, a sense of the end of an era in the history of the French Left.

WILLIAM LOGUE  
Northern Illinois University

JONATHAN BROWN. *Velázquez: Painter and Courtier*. New Haven: Yale University Press. 1986. Pp. xi, 322. \$45.00.

In this beautifully illustrated book Jonathan Brown reexamines the career of the great Spanish painter Velázquez and in so doing presents conclusions drawn from his decade-long preoccupation with the court of Philip IV. Essentially employing a biographical method, Brown retells the oft-told story of the artist's activity with close attention to the known documents and at least some discussion of all paintings he considers autograph. Dominant, however, is his extended emphasis, particularly in the second half of the text, on the character and significance of Velázquez's activity as a courtier. This emphasis defines Brown's chief contribution to the ever-growing literature on Velázquez and will render his book useful to scholars of European court cultures as much as to students of the history of painting.

The author states in his introduction what he considers to be the great theme of Velázquez's life,

the seemingly irreconcilable dilemma between his desire to be a great artist and his equal, indeed overriding, ambition to be a great seventeenth-century Spanish gentleman. The facts, particularly of the artist's final fifteen years, sustain such a perspective, but even Brown cannot escape the sense of tragedy that its full elucidation conveys. As he moves into his final and most eloquent chapter, devoted to *Las Meninas*, Brown exclaims, "How much better it would have been had the artist stayed at his easel, where he belonged" (p. 240)! Although agreeing with this lament, one applauds the author's insistence on adhering to the historical reconstruction he has undertaken, for it provides Velázquez, once and for all, with as majestic a setting as he himself created for Philip IV. Interpretive study of the paintings Velázquez produced within that setting will, however, continue, both in spite and because of Brown's book. Surely, Brown would agree that Velázquez intended it that way.

The book is divided into nine chapters (the tenth is really a four-page epilogue), which unfold chronologically and trace the artist's development from the early Sevillian works through his late masterpieces. In treating the early works, Brown incorporates the context of academism surrounding Velázquez's apprenticeship in his father-in-law's studio and also emphasizes the current of Italianism introduced into Spain by Philip II. He ranges himself among the anti-Caravaggists in discussion of the style of these early paintings but otherwise refrains from adding to the literature, suggesting only that their brilliant realism is "audacious but overreaching." Chapters 2 and 3 establish the circumstances of Velázquez's appointment as court painter in 1623, describe the works (mainly portraits) he produced during the 1620s at Madrid, and lead into the first trip to Italy and the emerging project of the Buen Retiro palace in the 1630s. Because the material on the Retiro appeared in Brown's study with J. H. Elliott, *A Palace for a King* (1980), the new material in these chapters derives from his positions on scholarly problems in these years. For example, he diminishes the strenuous emphasis José López-Rey laid on the portraits of Philip IV as projections of divinity (p. 52), accepts the importance of Rubens's presence in Madrid for the young Velázquez (pp. 65–68), and isolates the complex images of *Joseph's Bloodied Coat* and *The Forge of Vulcan* as evidence of Velázquez's aspiration to "the grand manner of Italian history painting" (pp. 71–78).

Chapter 4, on the "images of power and prestige" Velázquez painted for the Retiro and the Torre de la Parada in the 1630s, essentially recapitulates the author's views as stated in *A Palace for a King*. He characterizes, somewhat questionably, the Torre portraiture as essentially a counterpart to the Hall of Realms at the Retiro and argues in favor of

Velázquez's authorship of the London hunting-piece, the *Tela Real*, an important detail insofar as the painting shows the only actual event from court life Velázquez produced. In chapter 5 Brown indicates that in nonofficial works at this same time, such as *Calabazas* or *Mars*, Velázquez reveals his greatest technical experimentation and freedom of artistic expression; Brown builds a case for the artist as, in Manet's words, the "peintre des peintres." But herein lies the tragedy, for just at this time Velázquez practically abandoned painting and became less an artist than a kind of curator-decorator for Philip IV. Drawing heavily on recent dissertations by some of his students as well as earlier research by Yves Bottineau and Enriqueta Harris, Brown details the result of this activity, most of which no longer survives, in the following three chapters.

In the ninth and last chapter, after summarizing the artist's final contributions to the rehanging of several rooms on the principal floor of the Alcázar, Brown turns to the two late masterpieces that have always enthralled modern viewers, *The Fable of Arachne* and *Las Meninas*. He passes over the complex painting of Ovid's tale relatively quickly, essentially agreeing with Charles de Tolnay's interpretation of it as an homage to the nobility of the art of painting and citing more recent opinions that it is also, indispensably, an homage to Titian. It is obvious that Brown's heart belongs to *Las Meninas*. He devotes half the final chapter to its presentation and reaffirms his conviction that Velázquez intended the painting as a demonstration of his worthiness for ennoblement, his greatest aspiration, which at the time was being cruelly thwarted by the aristocratic membership of the Order of Santiago. In maintaining this position, Brown carefully assesses recent alternative interpretations; his rejection of the continuing attempts to employ perspectival systems to explain the role of the mirror reflection is particularly intelligent.

Ultimately, perhaps Brown's greatest service is the eloquent restraint he brings to his scholarly discussion, particularly of this great picture. It is both a moving tribute to and wise recognition of the genuine transcendence of Velázquez's art.

MARY CRAWFORD VOLK  
Harvard University

JULIÁN CASANOVA. *Anarquismo y revolución en la sociedad rural aragonesa, 1936–1938*. (Historia de los movimientos sociales.) Madrid: Siglo Veintiuno de España. 1985. Pp. 368.

Fifty years after its outbreak the Spanish Civil War is still being fought out on paper. That supporters of Franco and of the republic should go on defending



their respective views is not surprising. More curious is the intensity with which a civil war within the wider war is still disputed. For adherents of the republic the search for reasons for their defeat remains paramount. Brushing past the central issue, the international context, they have centered their acrimonious debates on the question of the primacy of war or revolution. For the anarchists the republic's greatest asset was the popular enthusiasm for a new world generated by the revolutionary conquests of the areas where the military uprising failed. For the Communists, the moderate socialists, and the liberal Republicans the revolution brought economic chaos and engendered the hostility of the democratic powers. It was thus a dangerous obstacle in the way of the main task, the military defeat of the insurgent Nationalists.

Whereas anarchist sympathizers produce romanticized accounts of the flowering of liberty and of popular consciousness and the Communists violent diatribes against the collectivist experiments, professional historians have tended to accept the views of one side or the other. The value of Julián Casanova's meticulously researched and provocatively written local study of the all-important Aragonese region lies in the way it supersedes cliché-ridden and often fictionalized accounts of a central issue of the Spanish Civil War. Anarchists and Communists alike, in their polemics, and most scholars, have accepted that the events of the summer of 1936 were in fact revolutionary. Casanova presents an entirely different, and convincing, view of practical day-to-day improvisation.

He skillfully dismantles the anarchist contention that, in the months immediately prior to the military uprising, popular militancy was ensuring that the establishment of libertarian communism in Aragón was imminent. Instead, he shows that the great majority of Aragonese villages had no unions of the anarcho-sindicalist Confederación Nacional del Trabajo (CNT), that labor disputes were limited to immediate economic aspirations, and that, in Zaragoza itself in the spring of 1936, the Local Union Federation of the CNT abandoned its revolutionary aspirations to make a pact with employers and the authorities on possible solutions to unemployment. More crucially, he demonstrates that, contrary to the conventional wisdom, the CNT in Aragón was extremely weak in rural areas and found the bulk of its support in the regional capital, the industrial city of Zaragoza.

The success of the military uprising in the western part of the region virtually wiped out the moderate socialist Unión General de Trabajadores. In the east, however, newly arrived anarcho-sindicalist militias from Catalonia dominated the process of collectivization. Once the insurgent forces were defeated, local antifascist committees sprang up in

every village. Sometimes known as defense committees or revolutionary committees, they were set up not out of revolutionary fervor but out of the necessity to fill the power vacuum. In some cases they had no more far-reaching ambition than to maintain law and order and prevent a bloodbath among the local people. On October 6, 1936, in the village of Bujaraloz in the province of Zaragoza, a meeting of delegates from the CNT unions of Aragón and from the militia columns set up a body to coordinate both the new economic structure and the war effort. Called the Consejo de Aragón, it was legalized by the central government on December 25, 1936.

The Council of Aragón was barely tolerated by the other political forces of the popular front, who regarded it as an anarchist dictatorship. It failed to control the random requisitions of the militia columns or to coordinate the economic activities of the collectives. An anomaly in the general process of centralization being pursued by the Loyalist government in Valencia, it was dissolved in the summer of 1937 not entirely, as Casanova makes clear, as a result of Communist repression. This splendid local study, which spreads light beyond the limits of its chosen region, is especially good in relating the problems of Aragón to the wider issues of the war and particularly to the political divisions in the central government. It is without doubt one of the most important contributions to the historiography of the Spanish Civil War to have appeared to date.

PAUL PRESTON

*University of London*

NANCY GINA BERMEO. *The Revolution within the Revolution: Workers' Control in Rural Portugal*. Princeton: Princeton University Press. 1986. Pp. xix, 263. \$28.50.

Nancy Gina Bermeo's book has many features to recommend it. It is well written and organized; it has good introductions and summaries and a more than adequate bibliography. It attempts to integrate theory with history in analyzing change in Portugal. Bermeo's most significant contribution, however, is her field work and extensive interaction with the rural people who were involved in the "agrarian reform," or the revolution within the revolution.

The study is divided into three parts: the emergence of workers' control in agriculture, the consequences of that control, and the problem of articulation or solidification its results. The reader is guided through the prerevolutionary history of Portugal to understand the setting in which the revolution of 1974 took place and in which the workers tried to take control of the land. The reader learns that attempts were made in the middle of the



twentieth century to increase wheat production and achieve self-sufficiency through nationalistic programs, such as the Wheat Campaign of 1929, and through government subsidies and backing. In this period ownership of land became more and more concentrated and the number of landless farm workers increased steadily, reaching over 80 percent of all cultivators in 1950 (p. 17). Radicalization of the workers in the southern region, in which the reform took place, preceded the revolution and stemmed from secularist tendencies, social interaction through the armed forces, and the migration of laborers to urban areas to escape rural unemployment. Political ideology, communist, socialist, or liberal, was important for rural change, but not as important as the rise of class consciousness and class associations in the following years. An important radical motto was "the land to those who work it."

Occupation of land by workers, which occurred in different phases, was an attempt to redistribute land, primarily uncultivated land, even if privately owned. The workers apparently were encouraged in this endeavor by the party in power and the armed forces, so that occupation generally took place without violence. This occupation led to self-employment and cooperative management, although no formal change in the tenure system or the title to the land occurred. The form of tenure that followed occupation is probably best described as squatting, which enabled successive governments to reverse the occupation movement only five years after its initiation. Apparently the results were especially easy to reverse because the workers themselves were not convinced that they should take away the land from its owners: they only wanted to be employed (p. 55). Therefore, the apparent agrarian revolution was certainly less revolutionary than is implied by the concept or by this study.

To explain the rise and fall of the land occupation movement, the author emphasizes the role of political parties, especially in the "articulation" or solidification of the results. Unsympathetic political parties in power hindered the articulation of the achievements, and therefore they were easy to reverse, as was done after 1979. The Communist party, which was the most sympathetic to occupation, was not strong enough among the workers or the policy makers to sustain the change. The author might have emphasized that the occupation was not based on a change of philosophy of tenure or of ownership rights and therefore was not adequately supported by political, economic, or social institutions.

This book is a pleasure to read, even though it has several problems. For example, it tries to cover too many topics, which tends to detract from the field work that is the most important contribution of the study. It uses concepts with little attempt to clarify

their meaning, including the important concept of agrarian reform; one could argue that no agrarian reform occurred—no legal change of tenure, no systematic change of scale or tenant-owner relationships or of methods of cultivation. Workers' control may be considered an improvisation to cope with a crisis, although certainly it is not unique in the history of agrarian change. Another conceptual problem arises from referring to "productivity" when the variable described is production. Although production increased by expanding the amount of land under cultivation, Bermeo adduces little evidence that factor productivity increased through occupation or workers' participation in management. The concept of workers' participation is itself unclear: was it participation by a representative of the workers, or was it management by the workers themselves? What about specialization and division of labor? Appendixes reproducing the main decrees and laws dealing with land in the period of the revolution would have been helpful; selective reference to these documents is not sufficient. Finally, even though many of the conclusions are impressionistic and may be hard to document, the book is a worthy contribution and a pleasure to recommend.

ELIAS H. TUMA  
University of California,  
Davis

JANE BLOCK. *Les XX and Belgian Avant-Gardism, 1868–1894*. (Studies in the Fine Arts, the Avant-Garde, number 41.) Ann Arbor: UMI Research Press. 1984. Pp. xvi, 185.

Les XX was an organization of Belgian avant-garde artists composed, as the name indicates, of a maximum of twenty members, which existed between 1883 and 1893. United not so much by homogeneity of style (it included realists, impressionists, neo-impressionists, and symbolists) as by shared antipathy to the salons and bourgeois society, les XX held its own annual exposition. Its success meant that in some sense it replicated the salon system it rejected. Les XX, initially composed only of Belgian artists, later admitted foreign members but regularly opened its shows to avant-garde artists from abroad.

Jane Block chooses to provide a "comprehensive view of Belgian avant-garde art from 1868–1894 and to relate its artistic aims to the political and social milieu" to study les XX as an organization rather than the work or character of its members (p. xiv). She adheres to this task, although she does not make much of an effort to evoke the political and social milieu. This omission reflects a more general weakness. The book has not attempted to draw in readers. It has severely circumscribed its audience by

presupposing both knowledge and interest in a group of artists who are mostly unknown (James Ensor is the only founding member widely recognized). The style is academic, most quotations are needlessly given in both French and English, and the occasional description of individual works is no more vivid than the murky black-and-white illustrations themselves.

This work contributes to a specialized field without appealing to any but the cognoscenti of Belgian nineteenth-century art history, whose number is few.

STEVEN PHILIP KRAMER  
Carnegie Endowment for  
International Peace and  
University of New Mexico

HARALD GUSTAFSSON, *Mellan kung och allmoge: Ämbetsmän, beslutsprocess och inflytande på 1700-talets Island* [Royal Officials, Decision Making, and Influence in Eighteenth-Century Iceland]. Summary in English. (Acta Universitatis Stockholmiensis, Stockholm Studies in History, number 33.) Stockholm: Stockholms Universitet; distributed by Almqvist and Wiksell, Stockholm. 1985. Pp. 317. 160 KR.

It is gratifying when a Swedish scholar studies an Icelandic subject, here "a case study of administration and power structure in Early Modern Europe" (p. 311). Harald Gustafsson had first to master the peculiar forms of Icelandic and Danish used in bureaucratic documents in the Kingdom of Denmark in the eighteenth century, and he certainly has done his homework.

Recently, Icelandic historians have paid little attention to the period from the end of the Middle Ages until about 1800. The current examination of the Danish monopoly trade by economic historians and the book under review are signs of increasing interest in this period.

In 1770 a royal commission was sent to Iceland. More than 1 percent of the male population responded to the commission's inquiries. Answers came from men of different social standing, although most came from the common people. This material is therefore eminently suitable for an evaluation of the influence of different groups on the decision-making process and not least for ascertaining the opinions of the peasantry.

With few exceptions the letters gathered by the commission consist of complaints about injustices and burdens exacted by the landlords and authorities. The officials, however, totally neglected these complaints. The results clearly show how the Icelandic officials guarded their interests and prevented social change. For prestige rather than financial gain they succeeded in retaining their exemption from

taxes almost unchanged. The Icelandic officials exerted a greater influence on the Danish government than any other group. Almost to a man they belonged to the families that possessed most of the privately owned land, and, as officials, they held the bulk of church and crown lands as fiefs. When the commission departed in 1771, the peasants had no further possibility of gaining the attention of the authorities in Copenhagen. Change first came with the rise of a middle class, based on the profits from activities that were forbidden to all Icelanders until 1787: fishing and trading.

Almost until this day Icelanders have looked on the period 1602–1855 through romantic glasses inherited from the ardent nineteenth-century advocates of independence. Until 1918 this was necessary for political reasons. But now the help of Gustafsson and others to adjust our understanding is welcome.

Gustafsson is to be congratulated for his feat. As he says, he was fortunate to have met the Icelandic historian Björn Thorsteinsson, master of an unending wealth of fertile ideas. For such an industrious and realistic scholar as Gustafsson it was an added bonus.

BERGSTEINN JÓNSSON  
University of Iceland

HIRO KAJANTO, *Porthan and Classical Scholarship: A Study of Classical Influences in Eighteenth-Century Finland*. (Suomalaisen Tiedeakatemian Toimituksia, series B, number 225.) Helsinki: Suomalainen Tiedeakatemia. 1984. Pp. 165.

Henrik Gabriel Porthan (1739–1804) is remembered today as the founder of modern studies in Finnish history and folklore, but from 1777 to his death he earned his living as professor of eloquence, (that is, Latin language and literature) at Turku (Åbo) University in the Finnish part of the Kingdom of Sweden. The critical edition of his *Opera Omnia* now being published has made possible a reassessment of his intellectual achievements. For his work as a classicist his own essays are supplemented by lecture notes and dissertations written by students under his direction.

Iiro Kajanto has defined his topic rather narrowly to include Porthan's use of Latin, his teaching of rhetoric and poetics, and his relationship to neoclassicist and neohumanist ideas coming from Germany, France, and England. Unlike some of his older colleagues, Porthan defended the utility of Latin as a scholarly language, and he carefully trained Finland's future clergymen in rules of classical rhetoric as an aid to preparing sermons in their rural parishes. His literary tastes were shaped by a moderate neoclassicism that valued clarity, har-

mony, and moral uplift over romantic subjectivism or witty satire.

Porthan was too deeply involved in the rational utilitarianism of the Enlightenment to share neohumanist enthusiasms for classical literature per se. He warmly echoed Johann Winckelmann's admiration for ancient Greece but seems not to have regarded Homeric poetry as a prototype for his own studies of Finnish folk poetry. Kajanto leaves this latter question dangling, however, and one wonders what indirect influence Porthan's training as a professional classicist may have exerted on the habits of mind he brought to his far more original work on Finnish traditional literature and language.

Those interested in classical studies after Porthan will wish to consult Pentti Aalto, *Classical studies in Finland 1828–1918* (1980).

ROBERTA G. SELLECK  
Harvard University Library

TORKEL JANSSON. *Adertonhundratalets associationer; Forskning och problem kring ett sprängfullt tomrum eller sammanslutningsprinciper och föreningsformer mellan två samhällsformationer c:a 1800–1870* [Nineteenth-Century Associations: Research and Problems Concerning an Explosive Vacuum or Principles and Forms of Organization between Two Social Formations, ca. 1800–70]. Summary in English. (Studia Historica Upsaliensia, number 139.) Uppsala: Uppsala Universitet; distributed by Almqvist and Wiksell, Stockholm. 1985. Pp. 321. 134 KR.

This is the first monograph to appear from the inter-Nordic group project "From Associations to Mass Organizations—Social Change and the Origins of the Modern Association Movement Studied in a Comparative Nordic Perspective" established in 1981–82 to examine the changes in the purpose, structure, and membership of organizations throughout Scandinavia during the great transitions of the nineteenth and early twentieth centuries. Torkel Jansson surveys the available literature on the development of associations chiefly in Sweden between ca. 1800 and 1880 and presents a number of case studies of associations that developed during that same period. In both respects, he has produced a solid, if not particularly exciting, work that provides an extensive introduction to the literature and a Marxist-based perspective on associational development.

The years Jansson covers mark the transition between the feudal, corporate society of late eighteenth-century Sweden and the capitalist, mass-organizational society of contemporary Sweden. He argues that this period was particularly "explosive," when groups in the society, particularly within the emerging middle class, sought to establish new or-

ganizational forms with which to meet a variety of problems within the so-called night-watchman state of the nineteenth century. So, for example, "voluntary associations" were established to deal with the stagnation of the state church, education, welfare, temperance, industrial development, cooperation, and the emerging working class. These associations were not, however, equal to the modern, mass organizations of contemporary Scandinavia—organizations such as the trade unions. They were undemocratic and controlled by elites. Their development reflected strong regional differences. Most "members" were only superficially involved in the associations.

In developing his argument, Jansson focuses on a number of specific associations, including the Evangelical Society, the Swedish Bible Society, the Women's Bible Society, the Friends of Destitute People, the Swedish Temperance Society, the Society for the Promotion of Monitorial Education, the Society for the Defense of Useful Knowledge, and the Swedish Industrial Association—all of which were founded between 1808 and 1845. In each case, he reviews the available literature and sources and introduces the founders, purposes, composition of the membership, and the distribution of locals.

Overall, this work is thorough, informative, and scholarly. Jansson's conclusions are carefully drawn. The Marxist argument that material conditions produce the other characteristics of a society underlies everything he has to say but is not forced and seems to fit quite well in this situation. The book concludes with an excellent time line, bibliography, and an extensive and well-written English summary. For readers interested in Scandinavian social history and in the modernization of the region, this is a beginning to what will probably be a significant group of monographs, articles, and dissertations to come from the project.

BYRON J. NORDSTROM  
Gustavus Adolphus College

MARGARETA R. MATOVIĆ. *Stockholmsäktenskap: Familjebildning och partnerval i Stockholm, 1850–1890* [Stockholm Marriage: Family Formation and Choice of Partners in Stockholm, 1850–90]. Summary in English. Stockholm: Kommittén för Stockholmsforskning; distributed by Liber Förlags, Stockholm. 1984. Pp. 390.

Margareta R. Matović's demographic history of family formation in early industrial Stockholm meticulously describes how marriage and premarital cohabitation preserved some remnants of the old estates society and helped form new industrial classes. To treat her hypotheses quantitatively, she leaves love in the background and uses social-

psychological exchange theory to demonstrate that marrying partners acted rationally to maximize assets.

Members of the middle and upper classes sought to combine a groom's economic resources and status with a young bride's dowry, fertility, and domestic skills. The qualities poor couples exchanged enabled them to survive in the crowded, expensive city. A working man preferred an experienced housekeeper approaching the end of her fertile years to a young, idle girl. A newly arrived country servant had no reputation to lose, and earnings to preserve, by avoiding marriage with her insecurely employed, often younger, man. A marriage law of 1734 reinforced the control of the husband-guardian over his wife's earnings. Thus, between 1850 and 1890, over 40 percent of those posting banns engaged in a "Stockholm marriage," perpetuating a rural tradition that regarded engagement itself as real marriage. Yet most of these people legalized their relations when they could afford to.

In short, the author describes two distinct subcultures of marriage. The first consolidated artisans and skilled and unskilled workers into a new, enormous working class. The other allowed aristocrats to cross class lines and marry the refined daughters of entrepreneurs and professionals. By 1880 most class barriers between the propertied and the unpropertied were firm. Matović's single most striking conclusion is that poor working women were reluctant to marry, because they cherished their independence.

The church books of Stockholm's eight parishes provide the rich materials for the author's statistical portrait of marriage in that city. Lutheran priests recorded birthplaces and dates, occupations, degree of Christian learning, the woman's sexual respectability, and premarital births when couples posted banns. People were amazingly forthcoming, even when they wished to appear a little better on the record. The author scrupulously evaluates the imperfections and lacunae of her sources. For the 1850s the author could find only one parish with enough information to correlate occupations, ages, and geographic origins with types of family formation. In the 1860s, when Sweden required churchmen to report annually to the Statistical Central Bureau, the information became more regular and detailed. For 1860 to 1890 she chose random samples representing 13 percent of those posting banns. To trace illegal cohabitation, not registered in church, she cautiously used housing records of couples who noted the woman as a fiancée or who registered young children. The number of these half-hidden relationships, she admits, is doubtless underestimated.

Although the author proves her hypotheses about class tendencies for marriages, her results seem

ironic. In the 1880s, just as the working class became more homogeneous, their behavior became more bourgeois. They married earlier and formed fewer premarital pairs. They gave their babies to foster homes and fetched them back only after legal marriage to declare them legitimate. To what extent then did parents' desire to legitimate their children encourage marriage? Did improvement in the married woman's legal status after 1872 or in the couples' prospects lead more workers to marry in the 1880s? Did maids absorb a bourgeois culture by working in middle-class households? These questions suggest that any exploration of human motivation, especially involving sexual life, demands an exploration of subjective testimony. Whenever the author includes a folk song or a selection from a novel to illustrate or introduce her interpretations, she provides fresh perspectives on her subject.

Yet just as it stands, with its tables of rich, precisely organized, clear data, this wonderful monograph can be offered to students of quantitative, family, and urban history. Michael Metcalf's concise English summary will prove helpful. In the future the author should enlarge this work with literary sources. It is already a model of care and logic producing significant results.

SONDRA R. HERMAN  
*Institute for Historical Study*  
*San Francisco, California*

KENNETH A. LOCKRIDGE. *The Fertility Transition in Sweden: A Preliminary Look at Smaller Geographic Units, 1855-1890*. (Report from the Demographic Data Base, number 3.) Umeå, Sweden: Umeå University. 1984. Pp. 140.

The aggregate demographic data for Sweden during the period of modernization are among the best in the world. Aggregate statistics for the national or provincial level do not, however, permit a refined analysis: the provinces are too large, inclusive, and internally diverse. Recently, some excellent studies have been done for the parish level using sources, such as household examination rolls (*husförhörslängder*), which give an annual census of each parish. But so far only ten or fifteen of the approximately two thousand parishes in Sweden have been analyzed in this tedious way. The combination of these local findings with an analysis on a wider scale has been difficult. Kenneth A. Lockridge's book represents a promising new compromise using middle-level aggregate data to analyze the fertility transition in Sweden. Until 1859 local ministers made statistical tables describing the parishes every fifth year. These tables were compiled to describe a deanery (*prästeri*), typically made up of three to seven parishes. Beginning in 1860 data were col-



lected and put together by the central authorities; the typical middle-level aggregate then became the *härads*, a judicial district smaller than a deanery, typically made up of one to six parishes.

In this study Lockridge singles out certain deaneries in 1855 as suspected focuses of early family-limitation behavior, chiefly by virtue of the relatively low fertility at any given level of nuptiality and by deduction of low marital fertility in these deaneries. Analysis of data on age-specific marital fertility from some of these geographical units in 1890 indicates that they were primary focuses of early family-limitation behavior in rural central Sweden. Yet these vital areas were structurally dissimilar. The only factor that seems to correlate with the advent of family limitation in all these areas is a wakened religious consciousness. From this the author concludes, in accordance with John Knodel's hypothesis, that cultural change was the vital factor in preparing for family limitation. This conclusion can be questioned, and Lockridge's reasoning is far from convincing. Moreover, since all theories on the fertility transition may, as the author admits, to some degree assume the shift from labor-intensive, family labor to a mobile market-orientated labor economy, cultural change alone does not explain the fertility transition. We must, however, agree with the author that further work with the Swedish statistics is needed.

The book is not overcrowded with formulas and definitions of technical terms. A few important concepts should, however, have been summarized in an appendix to make the book more accessible to the nonspecialist.

LASSE CORNELL  
University of Göteborg

PERTTI LUNTINEN. *F. A. Seyn: A Political Biography of a Tsarist Imperialist as Administrator of Finland*. (Studia Historica, number 19.) Helsinki: Suomen Historiallinen Seura. 1985. Pp. 343.

Pertti Luntinen's political biography of Frants Aleksandrovich Seyn, the tsar's last governor-general in Helsinki, is based primarily on materials in the Finnish National Archives. Thoroughly familiar with recent historiography on nineteenth-century tsarist bureaucracy, politics, and social and military history, Luntinen offers a balanced, scholarly study of a Russian administrator who was extremely unpopular in Finland, as director of Finnish Governor-General N. I. Bobrikov's chancellery from 1900 to 1906, as assistant governor-general between 1907 and 1909, and as governor-general between 1909 and 1917.

As director of Bobrikov's chancellery, Seyn was associated by the Finns with the harsh administra-

tive and police measures that Bobrikov introduced in Finland between 1899 and 1904. After 1907 Seyn symbolized the resolve of P. A. Stolypin's government to subordinate the local interests of Finland to the more general ones of the empire as a whole. As governor-general, Seyn, judging by the evidence Luntinen presents, had relatively little freedom of action, for the main lines of Finnish policy were determined by the Council of Ministers, the Duma, and specialized committees on Finnish affairs. Seyn, however, heartily endorsed and energetically carried out Stolypin's program of reform for Finland, which the Finns considered a violation of their constitutional rights. Only during World War I did Seyn show some independence in opposing measures of the Russian military authorities that he considered unwise or likely to diminish his authority as governor-general. He even justified such opposition to military policy in Finland by appealing to Finnish law. "It seems," Luntinen writes, "that wartime taught Seyn the skill of governing" (p. 254).

Seyn's reputation among Finns did not, however, improve during the war. Although he tries to be fair, Luntinen seems to share this negative view of Seyn held by Finns before 1917, for he describes Seyn as being "repulsive to normal civilized people not only because of his personal characteristics, but because of his gendarme connections" (p. 128).

Luntinen's study is carefully written in a readable, standard English (although the printer's typographical errors are distracting at times) and seems to have been intended for a non-Finnish, international audience. If so, the study should have clarified the fundamental revision of Finnish historiography on Russo-Finnish relations that has taken place since the 1960s. More pages on this subject and fewer pages on general Finnish and Russian history would have helped non-Finnish readers better understand the main issues that separated Finns and Russians during the years 1900–17. Yet the importance of Luntinen's study is to be emphasized, for in it he has demonstrated that much can be learned from the official Russian papers stored in the Finnish National Archives about how the Russian empire and its western borderlands were governed during the period 1809–1917.

EDWARD C. THADEN  
University of Illinois,  
Chicago

ULF LINDSTRÖM. *Fascism in Scandinavia, 1920–1940*. Stockholm: Almqvist and Wiksell; distributed by Humanities Press, Atlantic Highlands, N.J. 1985. Pp. vii, 196. \$19.95.

"Much ado about nothing" seems applicable to Ulf Lindström's study. First of all, the Scandinavian



countries experienced almost no fascism, despite the later use of "quisling" as a synonym for fascist and traitor, and the question of why must be answered. Fascist activity occurred in central and southern Norway, in southern Jylland in Denmark, and in the environs of Gothenburg in Sweden.

Rightist groups reacted most to their loss of power, the spread of social democratic (labor) strength, and the economic consequences of the depression. The existence of these reactionary activists virtually ended with the fabrication of agreements between the Marxist parties and the farmer's political organizations that lasted until the coming of war in 1940. Lindström admits that his analytical model does not reach the core of the problem; half of the work is merely description of politics and of labor movements and parties.

Why does Lindström provide no definition of fascism to distinguish it from reactionary rightist ideologies? Why does he confuse industrialism with capitalism? Industrialism was widely opposed, but capitalism encountered little resistance because of its existence for two hundred years or more. A whole section is spent on the working class, which did not support fascism. Racism in the period between the wars was part of the *Zeitgeist*—witness its presence in the United States. More space could be devoted to consensus, which was part of the theme of the 1930s in Scandinavian politics; for example, Dankwart Rustow's *Sweden: The Politics of Compromise* (1955) is neither cited nor used.

The work has merits, and it is the only book in English or a Scandinavian language on the subject. But a thorough proofing would have helped, for there are innumerable errors. An index would aid the bibliography to fit academic dimensions and requirements. Although the work has faults, it gives a general idea of rightist and fascist movements in Scandinavia.

RAYMOND E. LINDGREN  
*California State University,  
Long Beach*

WILHELM AGRELL. *Alliansfrihet och atombomber: Kontinuitet och förändring i den svenska försvarsdoktrinen 1945–1982*. [Nonalignment and Nuclear Weapons: Continuity and Change in Swedish Defense Doctrine, 1945–82]. Summary in English. Stockholm: Liber. 1985. Pp. 408.

Wilhelm Agrell's book on Swedish military doctrine from 1945 to 1982 is an important piece of research that examines the dilemmas of defense in a small, neutral country, caught between the two major power blocs in Europe and located close to one of the global powers. Military doctrine must reflect geopolitical realities, but it also becomes an instru-

ment of internal decision-making processes and procedures, so that the finished product will reflect the dynamics of the activity of pressure groups as well as strategic analysis. This is particularly so in Sweden, where the strategic analysis of the military professionals has often conflicted with elements of analysis produced by parliamentary supervisory bodies and other public organs. Furthermore, strategic debate in the United States during this period had a considerable impact on Swedish thinking, as did Soviet behavior in various parts of Europe and elsewhere in the world.

Despite the vicissitudes of internal politics and external influences, Swedish thought and actions in this area consistently focused on a few major goals and objectives throughout the period discussed. First, the Swedes wanted to stay out of a future war in Europe and, having chosen neutrality, had to make sure that this was feasible. This consideration led to the next: Swedish military capabilities had to be such that any would-be aggressor would hesitate before attacking; aggression against Sweden as a secondary target had to be so costly that the aggressor would refrain from it, because the cost-benefit ratio would be unacceptable. All other thinking about defense in Sweden since World War II stemmed from these givens. For example, the debate over possible acquisition of atomic weaponry took place within these parameters, and the decision not to produce and stock such an arsenal resulted from the emerging conviction that any attack on this relatively remote country would be conducted with conventional weapons and on a limited scale. Political realism also influenced the reasoning of military thinkers; to obtain necessary support in Parliament and elsewhere for atomic weapons would have been difficult. Ultimately, then, the Swedes settled for a satisfactory defense—one that had limited capabilities but was, nevertheless, sufficient to prevent war.

The book is interesting but is written in a difficult style. Many themes are repeated, the language is so "academic" that it becomes unduly cumbersome, and some points are belabored in excessive detail. But these are handicaps of style and language. Substantively, the book has much to offer, for it attempts to use various analytical models of decision making to examine this important topic. The reader who makes it through the entire volume will have learned a great deal about the specific case of Sweden as well as the more general problems of armed neutrality, decision making for defense in situations of economic scarcity, and the process surrounding the political game on this topic in pluralistic societies.

The book is well footnoted, often from original sources, and has a fairly detailed index.

TROND GILBERG  
*Pennsylvania State University*

BODO ROLLKA. *Die Belletristik in der Berliner Presse des 19. Jahrhunderts: Untersuchungen zur Sozialisationsfunktion unterhaltender Beiträge in der Nachrichtenpresse*. (Einzelveröffentlichungen der Historischen Kommission zu Berlin, number 51.) Berlin: Colloquium. 1985. Pp. x, 499. DM 128.

During the nineteenth century the feuilleton—that section of newspapers devoted to book reviews, fiction, literary criticism, and entertaining feature articles on recent cultural developments—became a standard and increasingly popular part of the daily press in Germany. Feuilleton journalism in the mass press has often been attacked, especially by the cultural and political Right. In the introduction to his novel *Das Glasperlenspiel*, for example, one of Hermann Hesse's characters condemns the "age of the feuilleton," when diversionary fiction and superficial cultural feature articles in the daily newspapers "were a major source of mental pabulum for the reader in want of culture"; the popularity of feuilleton "chitchat," he charges, was rooted in the bourgeoisie's "deep need to close their eyes and flee from unsolved problems and anxious forebodings of doom into an imaginary world as innocuous as possible." More prosaic German conservatives have traditionally regarded the feuilleton as a forum for subversive ideas and have often denounced feuilleton journalists ("Literaten") as dangerous radicals. Many scholarly treatments of the nineteenth-century feuilleton agree on its important role in shaping and giving voice to liberal public opinion, especially in the Vormärz.

Bodo Rollka now offers us a bold new interpretation of the history and role of the German feuilleton. His ambitious and copiously researched study, which examines the feuilleton literary sections of some seventy Berlin newspapers between 1810 and the 1870s, discusses such varied questions as the nature of the feuilleton readership, how newspapers used their feuilleton sections as a competitive commercial weapon, the evolving popular image of feuilleton journalists and of popular reading matter in general, and the various ways literature was discussed and transmitted by the Berlin press.

Rollka's primary interest, however, is in the function of entertainment literature as an agent of political socialization and in its ultimate influence on public opinion. He argues that literary discussions in Berlin newspapers became increasingly politicized during the nineteenth century and were an "important factor . . . [in] the formation and simultaneous canalization of broad political participation in social life" (p. 437). However, contrary to the widespread view that feuilleton journalism was an early outlet for oppositional sentiments and that it generally championed liberal political opinion, Rollka main-

tains that the literary sections of most Berlin newspapers (even the liberal ones) served far more to justify and perpetuate the existing sociopolitical order than to challenge it. At times quite subtly or unwittingly, at other times overtly and deliberately, Berlin's literary feuilletonists pursued a decidedly conservative "Literaturpolitik." On the one hand, they promoted "literature" as an asylum or refuge from political life and thereby fostered an escapist, unpolitical outlook among their wide readership that reinforced the status quo; on the other, their reviews and articles encouraged the consumption of popular literary works that preached reverence for tradition, identification with the state, and the need to preserve social harmony and the existing social structure. As Rollka rather inelegantly puts it: "Characteristic of the general tendency that defined the transmission-process of the news press in the field of literature, from the classics up to colportage, was the ever-manifest attempt at an exorcism—and thereby at a substitute solution—of real societal antagonisms through poetical form, in the interest of consolidating the existing relations . . . It [that is, the process of literary transmission through the nineteenth-century Berlin feuilleton] thereby contributed to the preservation of existing conditions, just as it did to the repression of those demands that verbalized the interests of oppositional minorities" (p. 437).

Not everyone will be persuaded by Rollka's arguments, for he tends to draw rather sweeping conclusions from a limited body of empirical data, and the inductive links between his generalizations and the particulars he cites are often unclear. His book provides, however, a wealth of important information about the social, political, and commercial context of feuilletonists in nineteenth-century Germany, and it should provoke a lively new debate over their intentions and political impact.

The author's impenetrable German academic prose deserves a final comment. Rollka's excruciating syntax, murky diction, and frequent digressions all present formidable obstacles to comprehension. His labyrinthine sentences are exhausting to read and often impossible to decipher; the book's stylistic abstruseness is further aggravated by his lamentable penchant for empty, abstract terms such as "Operationalisierung," "Aktualisierung," "konkretisieren," and "thematisiert."

GARY D. STARK  
University of Texas,  
Arlington

CORNELIA ESSNER. *Deutsche Afrikareisende im neunzehnten Jahrhundert: Zur Sozialgeschichte des Reisens*. (Beiträge zur Kolonial- und Überseege-

schichte, number 32.) Stuttgart: Franz Steiner, 1985. Pp. 235. DM 42.

Among the most significant additions to the historiography of modern imperialism in recent years has been a number of sociological studies of the European colonial services. In this, the latest volume in the series *Beiträge zur Kolonial- und Überseegegeschichte*, Cornelia Essner gives us an excellent analysis of another group whose activities in the nineteenth century strongly influenced the course of European imperialism: the people whom Essner calls "Afrikareisende," a category that includes explorers of Africa and certain other types of European traveler in Africa. Explorers have been studied collectively before, but never has so thorough and systematic an investigation been undertaken of the social context in which a particular national group of them—in this case the Germans—operated. The book might have been improved by more extensive comparisons with French and British travelers, but concentrating on the Germans allows Essner to go beyond her subject, to say a great deal about German academic life in the nineteenth century and about relationships between politics, science, and German imperialism. The book does not have a single major theme or an integrated argument, but the author's intelligent analyses of the various factors affecting the actions of the German travelers more than compensates for her somewhat diffuse presentation.

After an introduction in which the author discusses the difficulty of defining the subject group, she examines the organization and financing of German exploration in Africa. She shows how it developed from occasional forays by wealthy amateurs and participation by German academics in British expeditions into systematic travel organized by geographical societies, and, finally, into government-sponsored travel, usually in support of colonial rule. The importance of national politics in shaping these changes is especially emphasized. The second major section of the book concerns the problematic relationship between African travelers and the increasingly specialized, professional social scientific disciplines, particularly geography. The heart of the book, however, and by far its most interesting part, is the third: an analysis of the social backgrounds of 109 nineteenth-century German travelers in Africa and of evidence concerning their educations, motivations, and careers. The collective analysis is supplemented by a more detailed examination of the careers of several prominent travelers, including Heinrich Barth, Georg Schweinfurth, and Carl Peters. Among other things, Essner demonstrates that most of these people were not misfits or outsiders who lived eccentric lives but rather academics looking for career advancement in a tight

university job market. The final section discusses the factors influencing public reception of published travelers' accounts and the ways in which those accounts evolved to suit their various audiences.

This is a first-rate study that will interest readers in many fields. It is short and yet full of information. Altogether, it is an excellent addition to the series in which it appears.

WOODRUFF D. SMITH  
University of Texas,  
San Antonio

S. H. F. HICKEY. *Workers in Imperial Germany: The Miners of the Ruhr*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press, 1985. Pp. 330. \$39.95.

In the era of *Alltagsgeschichte* studies that do not isolate daily life from the organizational and political realities of the surrounding world are a pleasure to find. Mary Nolan's *Social Democracy and Society* (1981) and Vernon Lidtke's *The Alternative Culture* (1985) are two fine departures from the current trend as is S. H. F. Hickey's excellent, well-researched analysis of miners in the Ruhr before World War I.

The book's central chapters investigate housing, religion, work in the mines, miners' strikes, and trade union politics. Each contributes to the thesis that the working class of the Ruhr was still in the throes of "creation"; division and fragmentation were the norm, solidarity the exception. Company housing projects usually separated the newer arrivals in the region from second- and third-generation miners and thus reinforced the ethnic split between Polish and German workers. Not only did religion divide Christian unionists and Center party voters from their Social Democratic counterparts but the religious community also served as a haven for uprooted peasants and Poles, once again dividing workers along cultural lines. Hickey at this point counters Nolan, whose work on Düsseldorf found that new residents were often the first to break away from the religious and cultural milieu they had unquestioningly accepted in the old neighborhood. Work in the mines created further splits, pitting old against young, hewer against hauler, experienced workers against newer employees, and German against Pole. Not surprisingly, imposing strike, trade union, or party unity was out of the question. The book strikes a natural, credible balance between daily experience on and off the job and the consequences of the conditions of everyday life for trade union and political movements.

No work is without flaws, of course, and Hickey's is no exception. The book is based far too much on evidence from Bochum to qualify as a regional case study. The author also pays too much homage to

David Crew's *Town in the Ruhr* (1979), which emphasized the solidarity of miners in Bochum, to be consistent with his own thesis. Hickey's discussion of miners' strikes, and, especially, work in the mines, seems particularly torn between these two viewpoints. He succumbs too often, moreover, to the temptation to present admittedly powerful evidence in indented block quotations. Some chapters average more than one per page. As difficult as this makes the reading at times, historians of the German labor movement cannot afford to put Hickey's book aside, for it makes a major contribution.

ERIC DORN BROSE  
Drexel University

ROBERT H. ABZUG. *Inside the Vicious Heart: Americans and the Liberation of Nazi Concentration Camps*. New York: Oxford University Press. 1985. Pp. xiii, 192.

Drawing on an oral history project at Emory University, Robert H. Abzug has constructed a riveting study of soldiers who liberated the major concentration camps in Germany. All too brief and lacking comparable testaments from Russians, this work nonetheless addresses issues that have disturbed scholars since the end of World War II.

Like Bruno Bettelheim, Abzug concludes that residents of Weimar and Dachau may have blocked out the horrors of nearby camps. Many Americans also engaged in variants of "psychic closing off." Jewish soldiers and writers such as Meyer Levin stared in disbelief, and battle-hardened warriors such as George S. Patton became physically ill at the sights in Buchenwald and Ohrdruf.

Some liberators felt guilty of voyeurism. Others were determined to learn. In April 1945 General Dwight D. Eisenhower, concerned that many GIs did not know what they were fighting for, ordered every soldier not on the front lines to visit the camps "to know what he is fighting against." Some units were so enraged by what they witnessed that they permitted inmates to massacre their former guards or carried out executions themselves.

Abzug salutes the indomitable will of victims who had been scarred and hounded through as much as ten years of imprisonment. Yet in this other world he found a disturbingly stratified society. Even after liberation, national and racial antagonisms persisted, abetted by an inefficient occupying army that responded positively to the cloying behavior of Germans and their one-time allies from the Baltic states, the Ukraine, and Hungary.

The most devastating images from this worthwhile study, however, come from photographs supplied by the Defense Audio-Visual Agency. Some have been seen before—child survivors in striped uniforms, disputed gas chambers in Dachau, bodies

stacked like cordwood. No one, however, in this age of skepticism or "revisionism" can look on the charred remains of human beings incinerated at Gardelegen or Thekla and wonder at the capacity of humans to do evil.

SAUL S. FRIEDMAN  
Youngstown State University

WILLY ALBRECHT, editor. *Kurt Schumacher: Reden—Schriften—Korrespondenzen 1945–1952*. (Internationale Bibliothek, number 107.) Berlin: J. H. W. Dietz. 1985. Pp. 1045. DM 76.

This is an extensive and very useful documentary collection, edited and with a long introduction by Willy Albrecht. Some of the documents, especially letters, have not previously been published.

The introduction is a chronologically reliable but essentially uncritical biography. Indeed, only on its last page does the editor conclude that Schumacher's policies with respect to reunification and European unity failed, because of the international situation and the pro-European enthusiasm of West German youth. In contrast, he declares that Schumacher's successes were to prevent the Soviets from forcing unification of the Social Democrats with the Communists in 1946 and the West German Basic Law from being overly federalist.

That Schumacher blocked the union of the Social Democrats with the Communists, as subsequent Western historians have agreed, is certainly true. His success regarding the Basic Law, in my judgment, is questionable and in any case far less decisive. The introduction does not treat why Schumacher's advocacy of a state-controlled economy did not succeed, and what would have happened if it had. Nor has the West German electorate ever accepted Schumacher's passionate view that the German Social Democratic party (SPD), as the only Weimar party that did not compromise with the Nazis, had a right to rule the Federal Republic—also a fact whose causes the editor has not analyzed.

Finally, Albrecht does not discuss another theory often used to explain in part the long time Schumacher and the SPD spent in opposition: their basic miscalculations with respect to Great Britain, which the SPD mistakenly thought would remain a strong European middle power; the United States, whose domestic and foreign policies Schumacher and the SPD increasingly, and unsuccessfully, opposed; and France, whose nationalism and modernization they underestimated.

The editor does raise another important question: was Schumacher really a German nationalist? His answer is essentially that, despite his life-long Lassalleian emphasis on the German state and nation, his opposition to European unity, and his



near-fanatical support for German reunification, which led many to believe that he was a nationalist, he in fact was not. All nationalism, especially German, is relative. At the very least, Konrad Adenauer's coldly realistic estimate that reunification was not and would not soon be possible and his firm commitment to Franco-German reconciliation and European unity were not only far more successful but more beneficent for Germany and the world.

A Kurt Schumacher society was recently formed in the SPD by the right-wing minority of the party to commemorate and cultivate his anticommunist heritage. To that part of his heritage the present SPD leadership professes still to adhere, although their bilateral negotiations with the Socialist Unity party can hardly make him lie more quietly in his grave. But his nationalism they seem to many, including me, to share indeed.

WILLIAM E. GRIFFITH

*Massachusetts Institute of Technology*

DOROTHEE BUCHHAAS. *Gesetzgebung im Wiederaufbau: Schulgesetz in Nordrhein-Westfalen und Betriebsverfassungsgesetz; Eine vergleichender Untersuchung zum Einfluss von Parteien, Kirchen und Verbänden im Land und Bund 1945–1952*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 79.) Düsseldorf: Droste, 1985. Pp. 337. DM 58.

This book is a product of the official Commission for the History of Parliamentarism and Political Parties. Dorothee Buchhaas compares the impact of the parties, interest groups, and churches on the genesis of the North Rhine Westphalian School Law and the Second Codetermination Law of 1952. Based on extensive documentation and archival material, the book is a contribution to the burgeoning West German historical literature on the political developments of the immediate post-1945 years. This scholarship particularly addresses the question of whether the old social and economic elites were restored to power or whether the emerging Bonn polity was totally different from the Weimar Republic. The author traces the prehistory of the two legislative enactments to the relevant Weimar policies. She thus affords us the opportunity to answer the question of a "restoration" on the basis of well-documented particulars, aside from the revisionist debates of the 1960s and 1970s, which obscure the issues. The author duly emphasizes the change from the Weimar *Modus operandi* of proportional coalition politics to the majoritarian politics of Bonn (at least at the federal level), which accounts for the bourgeois coalition that passed the Second Codetermination Law in contrast to the grand coalitions still common at the *Land* level at that time.

Buchhaas, who has written another book on the evolution of the Christian Democratic Union (CDU) from a "*Volkskirche*" to a *Volkspartei*, expertly handles the complex political settings of partisan and intraparty conflict and interest groups.

The Weimar debate over the secular, interdenominational, or confessional character of the elementary schools not only led to painful polarization and the failure of the Reich school bill but also was instrumental in the breakdown of the consensus among the three republican parties. The issue was hardly resolved by the concordat of 1933, which traded Vatican recognition for insincere Nazi concessions on the schools. Thanks to the moderation of liberals and socialists, the post-1945 alignment over the issue of confessional schools was a great deal less polarized than under Weimar, although the Christian Democrats were swept along by the wave of popular religious revivalism and never intended to compromise on their position. Led by well-organized Catholic representatives who thought of the collapse of 1945 as "God's judgment for our godlessness" (p. 77) and encountering little interference from Protestants and from the British occupation, the CDU and the Center party immediately pressed for the restoration of confessional schools in Westphalia and the Rhineland and drove home their point in the public and legislative debates—not to mention huge Catholic rallies—of 1950–51.

The story of codetermination goes back even farther—to the days of World War I and the plant councils of the Weimar Republic. By 1949 the liberals (Ludwig Erhard) and conservatives among the CDU and its coalition partners, the Free Democratic party (FDP) and German party, were united in their opposition to codetermination at the plant level, whereas Christian and socialist trade unions, the British occupation, and some industrialists had come to favor it—the industrialists often to stave off decartelization or other painful interventions of the occupying power. Even the pope intervened in the debate over codetermination among Catholics—on the side of the conservatives. Occupation influence and Franco-German relations still left their marks on the First Codetermination Law (1951), which was passed by an ample *ad hoc* majority of the German Social Democratic party, the CDU, and the Center party. The subsequent deterioration of the relationship between Chancellor Konrad Adenauer and the trade unions made inevitable a far weaker, if more general, Second Codetermination Law without the votes of the opposition.

Both the North Rhine Westphalian School Law and the Second Codetermination Law were weighed down by the divisive Weimar legacy, the author concludes, even though the establishment of the federal government in 1949 foreshadowed new



alignments and conditions. The dominant position of the federal chancellor, in particular, became the fulcrum of most future policy decisions. At the same time, and in spite of Minister President Karl Arnold's efforts toward a Weimar-type grand coalition in the largest state of the Federal Republic, Adenauer succeeded in preventing North Rhine Westphalia from developing into another Weimar Prussia. The reaction to the totalitarian experience of the Third Reich, finally, had persuaded all groups and parties—except for the Catholic church—of the virtues of moderation and democratic compromise, and even the Catholic triumph of the School Law soon began to prove hollow with the incipient disintegration of this subcultural milieu. Buchhaas's incisive analysis of this crucial transition from past to present deserves our unreserved enthusiasm.

PETER H. MERKL  
University of California,  
Santa Barbara

DIETRICH STARITZ. *Geschichte der DDR 1949–1985*. (Neue historische Bibliothek.) Frankfurt a/M.: Suhrkamp. 1985. Pp. 278. DM 16.

DIETRICH STARITZ. *Die Gründung der DDR: Von der sowjetischen Besatzungsherrschaft zum sozialistischen Staat*. (Deutsche Geschichte der neuesten Zeit vom 19. Jahrhundert bis zur Gegenwart.) Munich: Deutscher Taschenbuch. 1984. Pp. 245. DM 9.80.

HERMANN WEBER. *Geschichte der DDR*. Munich: Deutscher Taschenbuch. 1985. Pp. 540. DM 19.50.

In the thirty years following the end of World War II, scholars rarely studied the history of East Germany, officially called the German Democratic Republic (GDR) since 1949. The cold war, Bonn's Hallstein Doctrine, and East Berlin's isolationism fed the West's ignorance of developments in the Soviet zone. East Germany was seen as a transitory phenomenon, and this view was reinforced by the Socialist Unity party's (SED) pledges of reunification and the Soviets' periodic initiatives to create a neutral, demilitarized Germany. When the divisions between East and West hardened into rival military and economic alliances in the late 1950s, the East Germans began to chart a course for the future by examining the past. In this sense, the building of the Berlin Wall in 1961 constituted the real founding of the GDR, for it forced Germans in the east to come to terms with their government and the East German state to seek a modicum of legitimacy among its population. In the mid-1960s East German scholars began to publish serious monographic studies and documentary collections on their country's past,

and, by the mid-1970s, they had created a kind of national history of the GDR.

The West Germans came to the study of postwar German history as slowly as did the East Germans and only undertook research on the GDR after the dramatic political developments of the late 1960s and early 1970s. The Basic Treaty of 1972 stabilized relations between the two German states. At the same time, the new foreign policy consensus in Bonn regarding the advantages of a flexible *Deutschlandpolitik* created an atmosphere that encouraged new contacts with and lively interest in the Germany "behind the wall." The GDR's relative economic success and political stability in the 1970s also elicited widespread interest in the Federal Republic and Western Europe.

Two of the Federal Republic's leading scholarly experts on the GDR, Dietrich Staritz and Hermann Weber, have recently published the first comprehensive Western histories of East Germany. The studies are skillfully documented and argued with great sophistication. They point to problems of evidence and alternative interpretations, and the writing is accessible and suggestive of broader historical perspectives. To be sure, the books have flaws. Weber's volume suffers from the periodic inclusion in his text of short, encyclopedic biographies that disrupt the flow of his argument, and Staritz's *Die Gründung* would be more useful with an appendix and fuller scholarly apparatus. Still, these books represent recent and contemporary history (*Zeitgeschichte*) at its best and can be enjoyed as masterful products of that craft.

Although the scope and intent of the books are similar, the style and even content of their presentation of the GDR's history are strikingly different. Relying primarily on East German secondary studies, Staritz tends to explore problems rather than explain developments. In this sense, his work is more historiographical as it attempts to elucidate the shifting self-image of the SED. A variety of factors—international and domestic—weave in and out of Staritz's history: inner-German relations and socio-economic development, the intransigence of the parties in Bonn, and Soviet involvement in East Asia. Weber's style is more explanatory, and his book has a stronger interpretive structure. Weber is also more explicitly interested in the development of politics and ideology in the GDR. As a result, he relies more heavily than Staritz on primary sources: the East German press and published documents, the archives of the Social Democratic party's (SPD) Ostbüro, and rare internal SED documents located in archives in Bonn, Bremen, and Mannheim.

Weber argues that leading German politicians and labor leaders in the Soviet zone initially sought to reconstruct society and the state in the spirit of Marxist socialism, which he defines as "the ideals of

equality and freedom" combined with the public ownership of the means of production (pp. 16, 494). At its founding in April 1946 the SED was still Marxist in this sense, thus maintaining the party's "German character" (p. 133). But the quickly escalating struggle between the United States and the Soviet Union narrowed the range of options for German socialism. In the East this meant that the SED was subjected to the increasing intervention of the Soviet military government (SMAD). As a result, by 1948–49 the SED was forced into the Stalinist mold of "a party of a new type."

Weber defines Stalinism as a peculiarly Soviet phenomenon, derived from "the backwardness of Russia" (p. 15). Essentially, then, the Soviet authorities and their German allies imposed an alien form of society and government on East Germany, which included typically Soviet Stalinist characteristics such as terror, purges, intimidation, and denunciations; the East German regime, therefore, was doomed to meet with the opposition of the masses. Later attempts to establish a modicum of independence from the Soviet Union, such as those of Walter Ulbricht in the late 1960s, were fated to be crushed by the Soviets. Even Erich Honecker's government, Weber argues, can best be understood as a return to Soviet orthodoxy, to subservience to the Soviet model and (un-Marxist) Soviet Communist ideas. The "real existing socialism" of the contemporary GDR is not socialism at all, Weber concludes; instead it "reduces to the all-encompassing power of the party or, more precisely, of its leadership" (pp. 494–95).

This simplified version of Weber's argument may be contrasted with Staritz's treatment. Staritz, for instance, does not look at the GDR's Stalinist "revolution" of 1948–49 as a drastic departure from the immediate postwar period. His chapter headings for the periods 1945–49 and 1949–53 are "On the Way to People's Democracy" and "The Contours of People's Democracy." Weber emphasizes discontinuity when he labels the earlier period "antifascist democratic 'transformation'" and the later "Stalinization." Staritz attributes the creation of people's democracies to shifts in the international situation, as well as to the dynamics of internal German politics. He is particularly critical of the West German SPD under Kurt Schumacher, which, he claims, left the SPD comrades in the East "in the lurch" (*Die Gründung*, p. 119). He also points to the strong antidemocratic traditions in the German Communist party that eased the transformation of the SED into a "party of a new type" (which Staritz calls a "Bolshevik type"). In general, Staritz considers the formation of the GDR to be the outcome of domestic and international forces as powerful as the influence of the Soviet Stalinist system. Rather than the Soviet model being imposed from abroad, "So-

viet experiences are carried over" to the GDR (*Die Gründung*, p. 135). Rather than presenting the SED as the handmaiden of the SMAD, Staritz notes that "the SMAD often only completed or legalized what the German administration . . . already had decided" (*Die Gründung*, p. 109).

Both Staritz and Weber assert that Western unresponsiveness to Soviet diplomatic initiatives in 1952, 1953, and 1955 contributed to the inclusion of the GDR in the Soviet bloc. Both also agree that the building of the Berlin Wall in 1961 greatly improved the GDR's sense of security and self-importance. But the authors treat the decision to build the wall differently, Weber emphasizing the Soviet role and Staritz the East German. Of the August 5, 1961, meeting of heads of the Warsaw Pact's Communist parties, Weber writes: "The conference agreed—in accordance with the Soviet conception—to building a wall" (p. 326). Staritz says of the same meeting: "The demand of the SED [to secure the border] was supported by the CPSU and finally agreed to by the [Warsaw Pact] party leaders" (*Geschichte der DDR*, p. 137).

Both authors describe Ulbricht's attempts to create a new model of socialist society in the 1960s, based on the successes of his program of economic reforms called the "New Economic System." Staritz contends that economic failures were as important in explaining Ulbricht's fall as political and ideological "offenses" against Soviet hegemony, factors more heavily emphasized in Weber's treatment. Staritz's reading of the Honecker years also depends more on an analysis of social and economic changes than does Weber's. The 1970s witnessed a new stage of the development of the GDR when the SED slogan of "the unity of social and economic policy" indicated real changes in the allocation of resources. The dominance of the technical intelligentsia during the late Ulbricht period was reduced, and the SED's new and successful housing and social programs won the adherence of workers. In Staritz's view, the problems facing the GDR in the 1980s are complex but not insuperable: how to guarantee a higher standard of living for all the population while maintaining incentives for technological innovation and personal initiative; how to motivate the population ideologically when Marxism-Leninism has petrified into a justification for a careerist, conformist party elite.

Weber, on the other hand, sees the GDR caught in an insoluble contradiction. An essentially Soviet-style apparatus and party dominate a society whose needs and desires derive from the highly developed economic system and labor culture of Central Europe. Built into this situation are irreconcilable conflicts with the population that will lead, in Weber's view, to continuing fluctuations between periods of stability and instability. Moreover, as long as the

GDR presents itself as a "socialist nation" it will have to face internal demands to live up to its Marxist ideological promise. An "anti-Stalinist but not anti-Communist" opposition will constantly reappear in a variety of forms, including inner-party struggles, and the "dictatorial" SED will have to crush it, just as it has crushed the opposition of this "third way" in the past.

Despite their differences in style and content, the books by Staritz and Weber constitute a milestone in the historiography of the GDR. They encapsulate decades of patient research and careful reconstruction of events by these authors and others in the Federal Republic, the GDR, and, to a lesser extent, in Great Britain and the United States. These volumes make clear that Western scholarship on the GDR can now provide important insights into the study of Eastern Europe as a whole.

NORMAN M. NAIMARK  
Boston University

A. JAMES MCADAMS. *East Germany and Detente: Building Authority after the Wall*. (Soviet and East European Studies). New York: Cambridge University Press. 1985. Pp. xi, 233. \$34.50.

The German Democratic Republic struggled to attain the degree of independence and self-confidence it enjoys today. Hampered from the start by oppressive Soviet controls and the seductive presence of its prosperous West German neighbor, the GDR had to gain the support of its own people, shed its role of subservience to the Soviet Union, and withal ward off the sometimes predatory West Germans.

A. James McAdams's study is the most complete yet to appear in English on how the GDR met this strategic challenge. Its strongest part looks at the years between 1961, when Walter Ulbricht walled in West Berlin, and the mid-1970s, by which time Erich Honecker's government was capitalizing on the recent four-power and inter-German accords. The wall stabilized environmental pressures sufficiently to permit Ulbricht to build the economy and his regime's authority. Ulbricht sought to attain these goals while ensuring that the GDR's socialist neighbors did not sell out its interests. He was fairly successful in sustaining a status quo based on insulating the East from the West—indeed, so successful that the Soviets eventually had to remove him to pursue their new goal of detente. Honecker then had to work within the framework of detente at once to expand the GDR's international involvement and sharpen its distinction from West Germany.

Although solid in its grasp of events and their implications for policy, the study does not escape the

difficulties faced by other explications of the policy process in closed societies: the lack of reliable evidence and weakness of existing theory. Thus, McAdams adopts a "rational-state-actor" model, in which personal and interest-related fissures within the policy-making apparatus do not exist, and all aspects of the making and implementation of policy are consistent with a firm, if flexible, line dictated by some overall plan. (Recent exposés of Soviet decision making in the KAL-007 and Chernobyl affairs indicate the model's problems.) Similarly, the ratio of hard facts and informed inference to reportage and speculation declines as we move from the 1960s (about which period much has been written) to the late 1970s and early 1980s.

In describing how Ulbricht and Honecker contended with a changing East-West environment to pursue their country's interests, McAdams's study is suggestive about how the GDR can solidify its position as a more autonomous member of the Soviet bloc.

RICHARD L. MERRITT  
University of Illinois

MARGARET L. KING. *Venetian Humanism in an Age of Patrician Dominance*. Princeton: Princeton University Press. 1986. Pp. xxi, 524. \$65.00.

Two developments have recently enabled historians to enrich the understanding of Italian Renaissance humanism. First, scholars have begun to pay serious and systematic attention to the political, social, and religious contexts within which humanists elaborated their ideas. Second, scholars have begun to look beyond Florence and to study humanist movements in Rome, Milan, Venice, Naples, and other cities. In combination these two developments have added considerable texture to the scholarly portrait of Renaissance humanism. With the book under review, Margaret L. King joins an illustrious group of analysts—including Charles Trinkaus, Lauro Martines, John O'Malley, John D'Amico, and Charles Stinger, to name a few—associated with one or both of these developments.

King's analysis fills three long chapters. In the first she establishes the social context of the humanist movement in Venice, where native aristocrats influenced cultural as well as political developments. Most of the Venetian humanists came from the ranks of the aristocrats, who naturally devoted their talents to the political, social, and ideological interests of their class and republic. Meanwhile, foreigners and nonnobles held posts as tutors, teachers, and secretaries at the pleasure of the governing aristocracy. Dominated as it was by the aristocratic citizenry, the social context of Venetian humanism thus helps explain the conservative, orthodox character

of the Venetian humanists' thought. This is the concern of King's second chapter, which analyzes the fifteen most important writings of Venetian humanists. Alongside Aristotelianism and Christian piety, King identifies subordination of the individual to the interests of the community as a prime trait of Venetian humanist thought. Humanist culture thus served as an intellectual bulwark for the values of tranquility and stability prized by the Venetian aristocracy; humanists in Venice produced almost no religious or social criticism, nor did they address the theme of human dignity that figured so prominently in other humanist centers. Finally, King outlines the course of Venetian humanism during the fifteenth century. She identifies terra firma conquests as the influence that originally put Venetians in close touch with cultural developments on the Italian peninsula and inspired the emergence of the first generation of largely patrician humanists. The second generation of Venetian humanists possessed impeccable cultural credentials, and, in the tradition of civic humanism, they vigorously represented Venetian interests in Italian politics. By about the 1460s, though, so King argues, the cultural synthesis of humanist talents and communal values began to break down; the third generation of Venetian humanists thus provided political and diplomatic services less readily than had their predecessors but instead occupied themselves more with scholarship and philology.

King's study rests on a thorough, systematic analysis of some ninety-two humanists active in Venice during the fifteenth century. The second part of her work most clearly reveals the extent of her research: it devotes almost two hundred pages to biographical and bibliographical sketches and to a technical discussion of the humanist community in Venice. Rich with information and bibliographical leads, this section alone will prove immensely valuable for future scholars working on Venetian humanism. Meanwhile, and more important, King's work will deeply influence scholarship on Italian humanism in general. Subtle and sensitive to nuance, attentive to political and social context, judicious and reflective in her interpretations, King has established a high standard for the analysis of Renaissance humanism. Complemented by similar studies on humanism in other cities, King's work makes a fundamental contribution to the understanding and characterization of Renaissance humanism as a cultural movement.

JERRY H. BENTLEY  
University of Hawaii

GAIL L. GEIGER. *Filippino Lippi's Carafa Chapel: Renaissance Art in Rome*. (Sixteenth Century Essays and Studies, number 5.) Kirksville, Mo.: Sixteenth Century Journal. 1986. Pp. 208. \$50.00.

In a movement away from the dominant attention given to Florence, recent trends in Renaissance scholarship have stressed the unique importance of Venice and Rome. These new interests are evident both in history and art history, and the themes that have proved valuable in one enterprise are also to be found in the other. Gail L. Geiger's study of the Carafa Chapel in Santa Maria sopra Minerva is very much a work in art history—full of stylistic considerations and artistic judgments—but Geiger is also closely concerned with the questions and themes that have come to dominate the interpretations of Renaissance Rome.

The chapel was commissioned by Cardinal Oliviero Carafa, a major figure in the church, an important patron of intellectuals, and a protector of the Dominican order. Carafa was a shrewd diplomat and politician, who used art to further his interests. To decorate his chapel, the cardinal selected Filippo Lippi, the son of Filippo Lippi. The younger Lippi, who worked in 1488–89 on the chapel, brought to Rome Florentine traditions he had learned from his father and Sandro Botticelli. But he was influenced by the Roman interest in classical antiquity and adapted his art to fit the new ideas in Rome. He also had to adapt his art to the special interests of his patron. Carafa was a major advocate of the study of Thomas Aquinas. He had been responsible for the annual sermon in the saint's honor given on his feast day, which marked the revival of interest in Aquinas among the humanists, especially those in Rome. To accommodate his patron's interest, the chapel that Lippi designed and executed was based on analogies from the three sections of the *Summa Theologica*. The program related closely to the doctrinal themes Aquinas had stressed and to Carafa's own religious hopes, since the chapel was to be the cardinal's funerary monument. In developing a complex iconographical and artistic style to incorporate his artistic talents and his patron's ideology, Lippi produced one of the major points of intersection between Renaissance humanistic artistic and religious thought and Rome's traditional, medieval-clerical authority. The union of these trends received its canonical form in the work of Raphael three decades later, but the Carafa chapel was an important harbinger.

In developing her arguments concerning the chapel's program, Geiger advances the work of John W. O'Malley, who has investigated in great detail the preaching of the humanists in Rome and, especially, their growing devotion to Thomas Aquinas as the most acceptable scholastic spokesman for Catholic doctrine. In this she is similar to Leo Steinberg, who used O'Malley's work as a starting point for his important (and I think not completely appreciated) study of the religious nature of Renaissance art, *The Sexuality of Christ* (1983). To a



great extent, therefore, this book is a gloss on O'Malley's theme of the integrative nature of renaissance religious thought. Geiger extends O'Malley's work by providing points where the orations he summarized might have provided the artist with the general tone of the work that Carafa favored. As a consequence, she notes, "Filippino's paintings reflect more doctrinal issues than was usually the case for a private chapel" (p. 49). Unfortunately, Geiger cannot demonstrate a direct correspondence between the chapel's program and specific humanists in Rome but rather posits a series of probable relationships that may serve as a type of conjectural proof on their own.

The editors of this series have provided Geiger good editorial support and abundant illustrations. Regrettably, no color illustrations supplement Geiger's descriptions of the paintings. Nevertheless, the book is a valuable addition to the growing material on the intellectual and religious society of Renaissance Rome.

JOHN F. D'AMICO  
George Mason University

ESTELLA GALASSO CALDERARA. *Un'amazzone tedesca nella Firenze medicea del '600: La Granduchessa Maria Maddalena d'Austria*. Foreword by GIORGIO SPINI. Genoa: Sagep. 1985. Pp. 178. L. 15,000.

The subject of Estella Galasso Calderara's study is a relatively unknown Habsburg archduchess, Maria Magdalene (1587–1631), the daughter of Charles, duke of Styria, youngest son of Emperor Ferdinand I. In 1608 Magdalene went to Florence from her native Graz to marry Cosimo II de' Medici, the fourth grand duke of Tuscany; she had borne him five sons and three daughters when he died from tuberculosis in 1621.

Much of the book is taken up with details of the four-year negotiations for the marriage, reports about each stop of her bridal journey, tiresome listings of the minutiae of the celebrations Florence staged for her reception and wedding and of subsequent mindless, though lavish, entertainments. This archival deadwood seems to have been put in to extend the study to book length because there was hardly any story to narrate. Florence, then an oasis of peace, withdrawn from the dynamics of European historical development, had lost its former creativity, its motion, its geniuses. The grand-ducal couple's life was equally bare of action, plots, ideas, or notable events. Magdalene was simply a good wife and mother. No scandal occurred in the Medici palaces in her time, nor did misfortune touch any of her children, who all reached maturity—a rarity then. As a widow, she spent her remaining years dressed in mourning; her religion and her family

duties sustained her. During Ferdinand's minority she acted as coregent of Tuscany together with her husband's mother, the dowager grand duchess Christina of Lorraine. When her son had taken office as Grand Duke Ferdinand II and her daughter Margherita had married Odoardo Farnese, fifth duke of Parma, Magdalene accepted the invitation of her brother, Emperor Ferdinand II, to a reunion in Vienna, but the arduous journey undermined her health. Unable to reach her goal, she succumbed to a high fever in Passau.

Regrettably, Calderara seems biased against the subject of her study. She disparages this innocuous woman in everything she was and did—or did not do. Of all the biting, undocumented remarks she heaps on her protagonist-victim, the unfairer is her derision of Magdalene's religious devotion as "bigotry" and "exhibitionism." Unable to understand that Magdalene's faith was sincere and that her fear of damnation was real, inbred from her childhood in Graz, Calderara applies today's terms to the time of the Counter Reformation at its apex. Magdalene's piety was shared and reinforced in Florence by her husband, his mother, and the atmosphere around them. But Calderara fails to acknowledge the effects of the spirit of reaction in seventeenth-century Italy—the consequence of Spanish domination and of papal-inquisitorial persecution. Instead, the author points to the grand duchess as being the cause that "soon Florence, that pearl of the Renaissance, would be obscured by the smoke of the most obstinate and gloomy Catholicism" (p. 62).

In his foreword Giorgio Spini praises his pupil's "delicious malevolence" with which only a "woman with her light touch" can depict so "entertainingly" the "failed life" of an "empty crowned head" (p. 12). Such a misjudgment compounds the impression that this study is, in fact, a failed historical biography. Magdalene's life was not a failure. She lost none of her children, and she left a succession of Medicis that secured Tuscany's independence until 1737, when the family "died out." Moreover, if the female line is given the attention it deserves, it will be seen that through their daughter Margherita, the duchess of Parma, the genes of Cosimo II and Magdalene lived on in the Bourbons and the Habsburgs. Not surprisingly, Calderara never mentions her grand duchess's important place in the genealogy of European royalty.

ERIKA SPIVAKOVSKY  
Westport, Connecticut

LAJOS PÁSZTOR. *La Segreteria di Stato e il suo archivio 1814–1833*. In two volumes. (Päpste und Papsttum, number 23.) Stuttgart: Anton Hiersemann. 1984–85. Pp. x, 348; vi, 352–564. DM 430 the set.



For nearly thirty years Lajos Pásztor labored in the Archivio Segreto Vaticano (ASV), for the bulk of that time as one of its select group of archivists. A prolific author, whose present work lists twenty-five of his own publications, he possesses an impressive familiarity with the holdings, organization, functioning, and modern history of the ASV. His two-volume examination of the secretariat of state and its archive is a detailed, one could say exhaustive, administrative history that brings together and expands on his previous studies of the subject. During period covered the secretariat's functions reached their widest extent.

Pásztor divides volume 1 into three parts. The first treats the responsibilities of the secretariat during the pontificates of Pius VII, Leo XII, Pius VIII, and the first years of Gregory XVI. The broad range of the secretariat's jurisdiction over internal as well as external affairs made the cardinal secretary a veritable prime minister. Ercole Consalvi powered the centralization that brought this situation into being; it persisted until 1833 despite opposition and projects for change.

In part 2 Pásztor focuses on the composition, organization, and functioning of the secretariat so microscopically that one finishes knowing the name, dates, salary, duties, and collaborators of every employee of the secretariat from the cardinal secretaries to the copyists. The reader will discover how different cardinal secretaries performed, how duties were assigned, how drafts were drawn up and initialed, and how reform plans were developed and aborted. Pásztor clearly reveals that the duties of those who wrote drafts, ciphered, copied, and classified were not rigidly compartmentalized.

His discussion of the archive itself, in part three, further emphasizes the danger of assuming that titles sharply defined function. The great reform of 1816 divided the holdings into two general groups, *interno* and *estero*, but the division between them was not rigid. A nuncio's dispatch could deal with a matter of domestic concern for the papacy and be so classified. A single document might treat several major topics, but it could be assigned to only one of the nearly three hundred rubrics into which the central holdings were divided. Pásztor's work will guide researchers through, or at least alert them to, the intricacies of the archives as they were organized in 1816. The author not only provides the names of all the rubrics for the archive and its related holdings but also briefly describes their content—a definite contribution. Since the organization of 1816–33 was reinstated in 1848, scholars working in the later period can also profit from Pásztor's study.

His second volume is an appendix, which, for the most part, presents stipend tables, reform projects, and photographs of drafts and finished dispatches exemplifying style and handwriting. The extensive

footnoting of volume 1 sometimes crowds the text completely off the page. Regrettably, the detail presented forced the abandonment of an early intention to carry the study past 1833, through the ephemeral reform of Gregory XVI, into the early years of Pius IX.

RAYMOND L. CUMMINGS  
Villanova University

FRANK M. SNOWDEN, *Violence and Great Estates in the South of Italy: Apulia, 1900–1922*. New York: Cambridge University Press. 1986. Pp. x, 245. \$39.50.

One of the perennial weaknesses of the Italian labor movement in the early twentieth century was its inability to penetrate the southern countryside. The one great exception was the region of Apulia, where militant rural labor organizations developed after 1900. Frank M. Snowden's deceptively titled book provides a compact but ambitious treatment of the origins, evolution, and ultimate demise of this movement in the decades leading up to the triumph of Fascism. In the process he also claims to shed new light on political violence, the interaction of landed elites and labor, and the social background of both revolutionary syndicalism and Fascism.

The great strength of the book lies in the first five chapters, which offer a social history of the society that gave rise to such a labor movement. Snowden excellently combines statistical data and oral histories to reconstruct the miserable everyday lives of agricultural workers in the late nineteenth century. As elsewhere in the south, the key productive unit in the countryside was the large estate (*latifondo*). In Apulia the *latifondo* was less a feudal holdover than the product of nineteenth-century land speculation. As a result, it produced conditions similar to those of the reclaimed swamp lands of the Po Valley, the other great center of rural labor militancy on the peninsula. With its single-crop economy based on wheat, the *latifondo* encouraged extreme seasonal employment and a radical simplification of the social structure. Snowden, however, does not limit his analysis to working conditions. Indeed, he includes a fascinating portrait of the urban horrors that also confronted laborers in the new agro-towns where extreme poverty and great wealth were dramatically juxtaposed.

Snowden rightly insists that the labor movement would not have arisen from these conditions without new ideas that transformed discontent into a conscious sense of injustice. In the case of the Apulian countryside, he emphasizes the mobilizing role played by the loss of access to common lands, emigration, anticlericalism, the left wing of the agrarian bloc, and urban workers and intellectuals.

Although the author's treatment of early union organizing is rather conventional and institutional in approach, his analysis of the landowners' reaction is quite insightful. He clearly delineates both the psychological and economic underpinnings of the intransigence of landowners by skillfully mining the private papers of the Pavoncelli family, one of the great landed dynasties of the region. The resulting confrontation with organized labor, which took place in an area of the country already infamous for extensive political repression and corruption, provoked levels of violence and coercion without parallel in the rest of the Italian countryside.

Unfortunately, Snowden's study loses both its momentum and its originality in the final three chapters. A mere seven pages are devoted to the war years, and they mostly repeat well-known generalizations about the Italian countryside as a whole. Likewise, the chapter on the period from 1919 to 1921 repeats a familiar story of the clash between popular expectations and postwar realities. Only the drought of 1920 and phylloxera seemed to distinguish the Apulian situation from the Po Valley in these years. Snowden's treatment of Apulian Fascism also does not add anything to the work done by Simona Colarizzi fifteen years ago. Finally, the book would benefit from a real conclusion or epilogue, telling us how the Fascist regime dealt with the social and economic problems of the region. These criticisms, however, should not obscure the genuine virtues of this book as a social history of labor militancy in the Italian countryside.

ANTHONY L. CARDOZA  
Loyola University  
Chicago, Illinois

JOHN F. POLLARD. *The Vatican and Italian Fascism, 1929–32: A Study in Conflict*. New York: Cambridge University Press. 1985. Pp. xiii, 241. \$34.50.

Italian church-state relations reached a watershed between 1929 and 1931. Yet, John F. Pollard observes, the specialist literature tends to dwell either on the *Conciliazione* of 1929 or the crisis over Catholic Action in 1931. His aim is "to trace and analyse the logical and organic relationship between the two events," hitherto only sketched in the general works of D. A. Binchy, C. A. Jemolo, P. Scoppola, and R. A. Webster (p. 13).

Genuine fears and illusory expectations dominate the story. The initiative for the Lateran Pacts lay with the Vatican, which saw its "Christian restoration of society" through nonpolitical means threatened by Italy's "fascistisation" in the late 1920s. In Fascist eyes, the *Conciliazione* was supposed to make the papacy into an "instrumentum regni." Nothing of the sort happened in international affairs; within

Italy the regime, after benefiting from clerical support in the plebiscite of 1929, was increasingly challenged by Catholic Action, especially its professional sections, which contained many reformist former *popolari*. This "Catholic offensive" peaked in 1931 when a conjunction of the Great Depression and the fortieth anniversary of the encyclical *Rerum Novarum* triggered extensive criticism of Fascist corporatism. Pope Pius XI anticipated such a Catholic revival under Article 43 of the Concordat, although he was apparently unaware of the widespread influence of the *popolari* in Catholic Action. But to Mussolini Catholic protests smacked of an anti-Fascist conspiracy. The consequence was governmental prohibition of the youth groups of Catholic Action, which in turn provoked the encyclical *Non Abbiamo Bisogno*. The sole weapon now left was repudiation of the *Conciliazione*, which neither party dared contemplate. By the September Accords of 1931 the pope tolerated Fascist "statutory," although the survival of Catholic Action's youth organization, however hedged, ensured a clerical victory from the post-1943 perspective.

Pollard has offset the inaccessibility of Vatican records with research in the copious Fascist police material in the Archivio Centrale dello Stato. Indeed, Italy's secular archives, the author avers, throw more light on papal than on Fascist decision making. The only source this reviewer would add to an impressive bibliography is Dino Grandi's diary for 1929–32, available on film in Georgetown University Library, Washington, D.C.

This cavil apart, the book demonstrates mastery of a wide range of material, which sustains a lucid delineation of factions within the Vatican and the Fascist regime. Thus, the crisis of 1931 is elucidated by "the virtual extinction of Clerico-Fascist influences in both Catholic and Fascist circles" and "partly [by] a revolt of the Catholic rank and file against the leadership, of *popolare* militants and other Catholic activists against the bishops and even against the Pope himself" (pp. 175–76). Altogether, this is a fine monograph with useful appendixes of translated documents.

ALAN CASSELS  
McMaster University

PÁL LIPTÁK. *Avars and Ancient Hungarians*. Budapest: Akadémiai. 1983. Pp. 207. \$19.00.

The protohistory and migrations of the peoples who came to compose the Hungarians pose a number of complex ethno-linguistic questions. In their habitat along the Kama and Belaya rivers in Bashkiria some of the southernmost Ugrians came into contact with Iranian and Turkic (largely Oghuric-Bulgharic) nomads and ultimately adopted their equestrian

steppe culture. Of the seven tribal names fixed in Hungarian tradition, most, like much of the Árpádian anthroponymy, are of Turkic origin. The names by which the Hungarians were known to the outside world are almost exclusively Turkic (compare "Hungarian" and "Ungar," which are derived from the Turkic "Onoghur"). The "proto-Hungarians" first appear in our sources in a largely Turkic milieu (Onoghur-Bulghar, Sabir, Khazar, Pecheneg). Their subsequent Danubian homeland already contained Avar, Oghuric, Iranian, and other populations that may have even included earlier waves of Ugrians. Given this powerful Turkic influence, reflected in a sizable body of Turkic loan words (which has provided gainful employment for generations of scholars), why do the Hungarians today speak an Ugrian language? What was the ratio of Ugrians to Turks in the complicated process of Hungarian ethnogenesis?

Pál Lipták's study attempts to provide some answers to these difficult questions by using physical anthropology to determine the various elements—themselves complex, multilayered entities—that participated in this process: "the contribution of the steppe peoples to Hungarian ethnogeny" (p. 30). Lipták's taxonomic review of the population of Hungary of the Avar period indicates that approximately 16 percent of the grave-finds belong to various branches of the Mongoloid race (subdivided into Baikalian, Sinid, Sayanic, and Central Asian). The Inner Asian origin of the ruling strata of the European Avars is beyond question. "Political" but not "ethnic" Avars found in sites not belonging to the upper stratum were Europoids of various types. Thus, relatively small numbers of Inner Asian nomads were able to impose their rule on larger populations.

The Hungarian story, greatly exacerbated by the relative paucity of material, is somewhat more problematic. Nonetheless, Lipták, confirming over a century of linguistic work, finds that the proto-Hungarians contained an important "Turanid" element, distinguished from their Volga Bulghar-Onoghur Turkic neighbors (with whom they mixed) only by the presence of a Uralian element. The Hungarian overlord class of the conquest period contained about 33 percent Turanid stock. This type is almost completely absent from the middle layer (warrior-military retinue stratum) and the common folk. This Turanid element, among whom the Europeo-Mongoloids (Mongoloids with strong Europoid admixtures) composed about 6 percent, provided the Turkic influence. The overlord class was primarily Turanid (of Western Eurasian extraction), Uralic, and Pamirian. The middle layer was essentially Mediterranean, Nordoid, and Pamirian with almost no Turanid and Uralic elements. The third layer, the common folk,

was also dominated by Mediterranean and Nordoid elements, alongside of other Europoids.

Clearly, then, although Turkic steppe elements dominated politically, non-Turkic elements were numerically superior. Although the overlord class was bilingual (Turkic and Ugric as we know from Constantine Porphyrogenitus), the Ugric speakers, whose language ultimately prevailed, were the majority.

This brief but succinct study, the result of decades of research, adds very important concrete data to an old and fascinating problem of Eurasian history. Students of proto-Hungarian and Eurasian steppe history will find this a most useful introduction to the physical anthropology of the region.

PETER B. GOLDEN  
Rutgers University,  
Newark

BOGNA LORENCE-KOT, *Child-Rearing and Reform: A Study of the Nobility in Eighteenth-Century Poland*. (Contributions in Family Studies, number 9.) Westport, Conn.: Greenwood, 1985. Pp. 170. \$29.95.

Bogna Lorence-Kot asserts that "in [eighteenth-century] Poland family behavior cannot be understood apart from politics" (p. 4), and her monograph faithfully pursues this theme. The focus throughout is on the Polish nobility, the *szlachta*, which in the period under discussion comprised about 8 percent of the Polish population (p. 162, n. 10). During the second half of the eighteenth century the *szlachta* was on the defensive against reformers for whom the values of *szlachta* culture had become outmoded, particularly when compared to the values of the West ("Vienna and Versailles"). The reformers challenged a wide variety of social attitudes, especially women's conception of themselves, their children, and their children's education, and hoped in due course to create a less family-oriented and more civic-minded population. Neither the *szlachta* conservatives (Sarmatians) nor the reformers were homogenous groups, however, and the author effectively shows the composition of each. Over the half-century when the disputes raged, both sides softened their attitudes (p. 112) and eventually achieved a kind of alliance. Alliance was clearly necessary, since the conflicts over reform within Poland were taking place under the gathering shadows of great-power expansionism. The subsequent emergence of Polish nationalism, embracing, as the author says, political radicals, moderates, and conservatives (p. 113), took place when Poland had already disappeared from the European map as a result of the partitions of 1772, 1793, and 1795.

The book contributes more to some fields of research than to others. Its value as an addition to

the literature on eighteenth-century reform movements cannot be doubted; on such subjects as educational reform, East European territories are virtually *terra incognita* to nonspecialists. It is also a useful addition to the comparative history of the attitudes of European nobilities; the chapters on adult-child relations, *szlachta* women, and *szlachta* education are especially informative. As a contribution to the general debate among social historians over premodern familial attitudes, however, the book is disappointing. The author deliberately sets the book (p. 3) within the debate over parent-child relations occasioned by Philippe Ariès in *Centuries of Childhood* (original 1960; English translation 1965) and, more recently, Linda Pollock in *Forgotten Children: Parent-Child Relations from 1500–1900* (1983). Yet no subsequent use is made of this framework or of the substantial literature it has engendered; what follows the initial pages is an internalist account of an aspect of Polish history. Moreover, the investigation of familial topics really requires more precise language than is evident here. Such expressions as “astronomical birth rate” (p. 24), “men waited as late as they could [to marry]” (p. 48), and “girls remained under maternal direction forever” (p. 23) are distracting and call into question how well the author has understood the larger debate. If the demographic information on the *szlachta* is as “spotty and unsatisfactory” as the author says (p. 16, n. 53) then observations that use such vague wording should be avoided until more is known. Finally, given the current state of knowledge, overconfident generalization should also be eschewed. Much more research in primary sources of a quantitative kind is needed before the reader can accept the author’s claim to be describing a “typical extended *szlachta* family” (p. 11) or believe that such a generalization as “in all sexual and social matters, class affiliation defined public response” (p. 59) is an empirically grounded statement and not a questionable surmise based on anecdotal evidence.

ANDREJS PLAKANS  
Iowa State University

KEITH HITCHINS. *The Idea of Nation: The Romanians of Transylvania, 1691–1849*. Bucharest: Editura Științifică și Enciclopedică. 1985. Pp. 220.

EMANUEL TURCZYNSKI. *Von der Aufklärung zum Frühliberalismus: Politische Trägergruppen und deren Forderungskatalog in Rumänien*. (Südosteuropäische Arbeiten, number 81.) Munich: R. Oldenbourg. 1985. Pp. 280.

Keith Hitchins’s collection of essays focuses on the development and emergence of the idea of nationhood among the Romanians of Transylvania during the eighteenth and nineteenth centuries. He traces

Romanian ethnic consciousness from its origins as a primarily religious conception to its emergence in the nineteenth century as a secular ideal. The initial prominence of a religiously oriented concept of nation was naturally related to the activities of the clergy-led generation of the eighteenth century that first “discovered” the main sources of Romanian national identity: the history of the Daco-Romans, the Latinate Romanian language, and Christianity. As the social, political, and ideological changes of the late eighteenth century and early nineteenth century took shape so did the appearance and rise to leadership among the Romanians of Transylvania of lay intellectuals. By the revolutionary year of 1848, Romanian national consciousness had become for intellectuals a largely secular concept.

At the same time, the other carriers of Romanian ethnic awareness, the Transylvanian Romanian peasantry, continued to embrace a “community” conception of nationhood that was a composite of religion, tradition, and custom. The contradiction between this religious ideal and the increasingly secular ideal of the intellectuals naturally led to conflict. On the other hand, the two contributed to each other in ways that had the overall effect of strengthening Romanian national consciousness rather than undermining it. Discussion of this interplay forms a second major focus of Hitchins’s book.

Finally, Hitchins’s essays concentrate on the impact on the Transylvanian Romanians, both intellectuals and peasants, of outside currents and ideas. Expanding opportunities for higher education, the Enlightenment, romanticism, liberalism, and socio-economic change in the Habsburg Monarchy all played a role in the shaping of the Romanian idea of nation, however much that idea was rooted in internal developments and forces.

Hitchins’s essays are what we have come to expect from the leading American student of Romanian history: extensively researched, thorough, and well written. They are an excellent supplement to his major books on the same era and a valuable resource for anyone who wants succinct treatments of most of the critical episodes in the emergence of the Romanian national idea in Transylvania.

Emanuel Turczynski’s study is an ambitious and so far unique attempt to formulate a comprehensive explanation of the emergence of nineteenth-century Romanian nationalism and political currents that takes into account developments in all of the Romanian lands. He begins with a survey of the political and social structure of each major region: the Danubian Principalities of Moldova and Muntenia, the Principality of Transylvania, the Banat, the Bucovina, and Bessarabia. Turczynski emphasizes the effects on each region of the different dominant empires (the Ottoman, Habsburg, and Russian) and the cultural-ethnic melange found



therein. An effort is also made to place the study of these, and subsequent, questions on a structural and statistical basis.

In the context of this analysis of regional structure, the remainder of the book focuses on other factors and developments that fostered differences between Romanians of each region and affected the development of national consciousness and political activity in these areas. Among these were the role played by social class in the emergence of national ideas, the varied impact of cultural currents because of their sources (for example, the Enlightenment's German source in Transylvania and its French source in Muntenia and Moldova), the attitude of Romanians in various regions toward the existing authorities and toward law itself, the role of religious leaders (though not of religion per se), the degree of ethnic conflict, contrast, and animosity, and the extent to which national and political ideas were reactive or initiatory.

Naturally, the subsequent political shape that national ideas and political currents assumed in each region showed considerable diversity. At the same time, the relatively high degree of exchange between Romanians of all regions contributed to an increasingly uniform sense of national consciousness. The fundamental linguistic and ethnic unity of the Romanian people, whatever the region, created this interplay of currents and ideas within a constantly shifting and enriched spectrum. The net result of the process was that, although Romanian national feelings differed from area to area, creating antinomies of various kinds between and even within regions, a relatively coherent national ideal emerged by the mid-nineteenth century.

Description, analysis, and explanation of this diversity and unity of the Romanian national consciousness is the most successful of the two major themes of Turczynski's study. This is largely because of his comprehensive knowledge of the Romanian lands and their histories and his ability to bring it together into a comparative study that few could even attempt. The other emphasis of this book, the attempt to go beyond the work of cultural and intellectual historians for a statistical analysis of the Romanian eighteenth and nineteenth centuries, is less successful. The kind of data necessary for convincing work of this type is simply lacking for Romania. The effort, nevertheless, is suggestive and provides both direction for future studies as well as perspective on other, more usual, approaches. The works of Hitchins and Turczynski show the increasing sophistication and skill with which Romanian studies are now being pursued.

PAUL E. MICHELSON  
Huntington College

WILLIAM W. MCGREW. *Land and Revolution in Modern Greece, 1800-1881: The Transition in the Tenure and Exploitation of Land from Ottoman Rule to Independence*. Kent, Ohio: Kent State University Press. 1985. Pp. xxii, 339. \$35.00.

This book attempts to illuminate Greece's agricultural economy by analyzing the transformation in land tenure following the establishment of the modern Greek state from 1800 to 1881. Breaking away from the Ottoman empire accentuated the pressure for the anticipated disposition of the former Turkish estates. William W. McGrew divides his manuscript into two parts. The first part incorporates aspects of the agricultural conditions evolving from Ottoman Turkish rule and describes revolutionary legislation, land value, and geographical distribution before 1833. In part 2 McGrew examines the government's controversial land policies for the years 1833 to 1881.

The background of land reform was a decade of revolutionary struggle, civil war, and anarchy that had physically exhausted Greece, leaving its towns depopulated and fields untilled. Regional and family loyalties, fed by the fires of revolt, had divided the Greek people into warring factions, each with its own solution to the nation's problems. Although the government desperately attempted to manage a situation it had neither created nor desired, political differences added to confusion and conspiracy. In this atmosphere government turned its attention to a noteworthy development in modern Greek history: the distribution of previously Turkish property. Immediate measures were needed to rehabilitate agriculture and revive transportation. McGrew indicates, however, that the agrarian issue was obstructed by illegal occupation of the public domain and interminable litigation. President Capodistrias tried to avoid useless squandering of state lands but fell victim to the destabilizing tactics of the opposition.

Beginning with the dotation law of 1835, and culminating with the enactment of the land distribution program in 1871, the central government responded to pressure by providing land grants. In this thoroughgoing apportionment, however, the governments were coping with a backward, underdeveloped agricultural economy. The deplorable condition of transportation, the disorganized communication system, the excessive demands of the tax farmers, and the indebtedness of the rural population weakened the effects of land reform. The peasants felt that the government's obligations were to protect and assist them but not to interfere through regulations and restrictions. The government, in turn, wrestled with the existing inadequate agricultural methods and inequitable tax structure. Whatever the problems engendered, the legislation



concerning land tenure represented the first comprehensive attempt to bring agricultural progress and fiscal reform to a battered nation.

The volume includes seven appendixes, three maps, six descriptive tables, a glossary, and an index. Extensive notes and twenty-seven pages of bibliography of published and unpublished primary sources testify to McGrew's familiarity with the pertinent literature and his patience and diligence in searching for neglected material. He has woven together the scattered evidence elicited from documents, memoirs, correspondence, contemporary accounts, newspapers, and articles. The author is forceful and judicious in his presentation, but, on occasion, he depends on personal judgment. Feeling that the last decades of the nineteenth century merit a separate stage and analysis, he excludes any discussion of the acquisition of the fertile Thessalian plains, even though it would have added more substance to the subject. These minor reservations, however, should not detract from the importance of the volume, which represents the first scholarly, lengthy account of the subject in English. Its major contribution lies in its comprehensive examination of land tenure.

WILLIAM PETER KALDIS  
Ohio University

TOMASZ SZAROTA. *Warschau unter dem Hakenkreuz: Leben und Alltag im besetzten Warschau I. 10. 1939 bis 31. 7. 1944*. Translated from Polish by CLAUDIA MAKOWSKI and RYSZARD MAKOWSKI. Foreword by WOLFGANG JACOBMEYER. (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1985. Pp. 365. DM 68.

DAVID WDOWINSKI. *And We Are Not Saved*. Including CHAIM LAZAR, *The Jewish Military Organization in the Warsaw Ghetto*. Foreword by MORRIS CHARITON. New York: Philosophical Library. 1985. Pp. xxi, 222. \$19.95.

Warsaw in World War II, with its unique experiences among the European capitals, has been an inexhaustible subject for historical writing, although most of it is in Polish. Recently, two books on the topic were made accessible to the Western reader. One is by Tomasz Szarota, who above all is known for his massive study, *Okupowanej Warszawy dzień powszedni* (The Daily Life of Occupied Warsaw), published in 1978. After a delay of seventeen years and in a beneficially abridged version, this work has made its appearance in German.

Using an impressive array of sources, Szarota surveys the existence of the city under the repressive Nazi occupation. His work does not cover the periods of hostilities—the siege of Warsaw of 1939, the Ghetto Uprising of April 1943, and the Warsaw

Uprising of 1944—which do not fit his notion of the city's daily life. The contents are arranged topically rather than chronologically, and the discussion is centered on such issues as how the inhabitants fed and clothed themselves, their living and working conditions, and how they spent their free time. The occupation led to a sharp increase of economic differentiation among the population, not only Polish but also Jewish in the Ghetto, with the war profiteers and black market entrepreneurs emerging as a new moneyed class. Despite the policies of the occupier that drove underground almost all artistic activity and educational work above the level of the vocational high school, the cultural life of the city was vibrant. The illegal press, literature, musical and theater performances, even sporting events, were all testimony to the efforts of the Warsaw intelligentsia, and in many ways Szarota's book is a tribute to the wartime saga of this social group.

Szarota also shows Warsaw's privileged community, the Germans, whose numbers included a few occupation regime officials and many transient soldiers passing through this largest staging point behind the eastern front. The personnel of the Nazi administration, with some notable exceptions, distinguished itself not only by brutality but also by corruption. According to widespread opinion, it was possible to buy any favor and to save anyone from jail, deportation, even from execution, for a sufficient amount of money.

In one part of his book Szarota breaks from the narrative of details and attempts a probe into the ultimate meaning of Nazi rule in Poland. He terms the essence of this rule a "sociopsychotechnic experiment," and this seems to be an appropriate keynote for the book. Yet, in this section on public morale and attitudes, he seems to be less at home than in the chapters studded with facts and data. This is clearly the least successful part of an otherwise remarkable work, and it may well serve as a reminder of the limitations of the traditional tools of the historian for confronting problems of sociopsychology.

David Wdowski's book deals not with Warsaw as a whole but only with one of its sections, the ghetto. Wdowski was by profession a psychiatrist, whose life, as he proudly declared, was shaped by Sigmund Freud, Theodor Herzl, and Vladimir Zhabotinsky. His intention was, obviously, not to write a history but rather to present a personal account of his experiences in the ghetto, and his book belongs to a vastly different category than Szarota's. He seems to confirm Szarota's observation that, although the living conditions in the Ghetto were incomparably worse than on the Aryan side, the Jews were given a larger measure of cultural autonomy. Life in the Ghetto was amazingly active in its intellectual manifestations. In their political attitudes, the majority hoped that obedience and submission would some-

how assure survival, and they rejected the idea of resistance.

Another dimension of political diversity, a cause of bitterness for Wdowinski, was the conflict between the General Zionists and the Revisionists, among whom he counted himself. Indeed, his book reads sometimes as much as an attack on the Zionist establishment for its policies of restraint as a recollection of his ghetto days. Although he was an advocate of armed resistance, he has little to say about the Ghetto uprising of April 1943 or the preparations for it, and the publisher saw it necessary to supplement the text with an essay on the Jewish Military Organization by Chaim Lazar. Like Wdowinski, Lazar traces the roots of the Jewish resistance to the thought of Zhabotinsky, the founder of Zionist Revisionism. In his view, the launching of the uprising was made possible by the fact that, after a series of mass deportations, those who remained in the Ghetto as the work force for the Germans were the young, physically fit, and politically conscious.

All told, the two books, so different from one another, provide ample factual information on Warsaw under the Nazis. Both also shirk an in-depth investigation of the all-important psychological climate of the time, and this unwillingness keeps them largely within the confines of factual narrative.

TADEUSZ SWIETOCHOWSKI  
Pilsudski Institute of America  
New York, New York

JOHN L. H. KEEP. *Soldiers of the Tsar: Army and Society in Russia, 1462–1874*. New York: Clarendon Press of Oxford University Press. 1985. Pp. vi, 432. \$55.00.

In the current military history "boom," this volume should be on everyone's list. John L. H. Keep is one of the Western world's foremost and most wide-ranging scholars of modern Russian history, with works to his credit on topics ranging from sixteenth-century Muscovite legal matters to a very distinguished book on the revolution of 1917. This volume further demonstrates the capacity of this historian.

The book divides into five chronological sections. First is a lengthy section on Muscovite military matters, from 1482 through 1682. In spite of its incorporation of some recent research, this section does not add very much to the existing literature on the subject. Next is a section on Peter the Great, who did many things that were both actually and allegedly innovative and left many records. Peter's activities also have been researched before, but Keep's is the best presentation in English of his military concerns. The third section, on the period

1725–1825, is certainly Keep's major contribution (sometimes based on his own archival investigations in the Soviet Union) and unquestionably the most interesting part of the volume. These were the years of the Petrine legacy, when the Russian army was the paramount force in Eastern Europe and also fought its way to Berlin in 1760 and Paris in 1814. The next period is that of Nicholas I and the bureaucratized parade-ground mentality that led to the debacle of the Crimean War. The last period is that after the Crimean War, personified by Minister of War Dmitrii Miliutin, when Tsar Alexander II directed the country from the depths of defeat and demoralization through a period of reforms that culminated in the military reform of 1874, which moved Russia into the modern world of military professionalism, a smaller standing army based on selective conscription, and large military reserves based on native manpower. Miliutin's reforms were, however, too narrow and failed to provide sufficient momentum for Russia to escape humiliation by the Japanese in 1905 and the Germans in World War I because of an inadequate industrial base, grotesquely excessive bureaucratization, incompetent officers recruited still too frequently from the gentry, inadequate reserves, and insufficient belief by both those reserves and the population at large that the regime was legitimate and worth fighting for.

This, of course, is not a book about the major battles and wars of the era, although they are not ignored. The evolution of military technology and its impact on the Russian army and society are also largely left to others' work. Keep's major concern is with the officers and men in the army. He portrays the change from the Muscovite elite cavalry army (a nonmilitant "warrior caste") assisted by relatively small numbers of infantry, to the imperial army consisting of an elite officer corps commanding a caste of brutalized near-slaves (mostly former serfs) serving for life, to a proto-modern army in which the distance between officers and men was greatly lessened. Keep shows how officers and men were created, how they lived (usually badly and often briefly), and, to a lesser extent, their impact on the rest of society, from which, until the 1870s, they were largely divorced. The relationship between the social status of officers and men and the success of Russian arms is not clear, but, when failure occurred, social relations undoubtedly were partly to blame.

The book is crammed with interesting subtopics and insights. One such topic, to which perhaps disproportionate space is devoted, is the notorious military colonies of the first half of the nineteenth century, often associated with A. A. Arakcheev, which were a variation of the government's constant attempt to make the army more self-supporting. A related theme is shortage of resources: Russia was a

comparatively poor country that fundamentally lacked the means to maintain a grandiose military establishment, and therefore both officers and men often suffered terribly in peacetime as well as at the front. Corruption added to the suffering of the common soldiers, as did their officers' inadequate education. Perhaps because of the large volume of Soviet research, Keep devotes attention to practically every known dissident movement and disorder that involved military men, from the Old Believer and Pugachëv movements of the seventeenth century, through the palace coups of the eighteenth century, to the Decembrist and Polish nationalist movements of the nineteenth century. Keep discusses the consequences of the fact that soldiers' wives were essentially widows but says little about the character of the men's lives under such a regime, which did not allow most soldiers to marry at all.

Keep's translations of Russian terms (and poetry) usually are exceptionally felicitous and followed by a transliteration in case anyone wonders about the original. Nevertheless, Muscovite terminology is still in need of standardization, and Keep's new offering of "junior gentrymen" for *deti boiarskie* (p. 23) (which probably did not "literally mean 'boyars' children'" [p. 28]) seems to be a movement in the direction of inaccuracy and barbarism, rather than clarity. Moreover, stating that *chernyi* in the context of taxpaying peasant means "black" (pp. 46, 103) probably does not help much either. Such blemishes detract little, however, from a magnificent book.

Oxford Press is to be congratulated for putting out a text with almost no printer's errors and with the footnotes at the bottom of the page.

RICHARD HELLIE  
University of Chicago

NEIL CORNWELL. *The Life, Times, and Milieu of V. F. Odoevsky, 1804–1869*. Athens: Ohio University Press. 1986. Pp. xiv, 417. \$38.95.

Neil Cornwell's study of Prince V. F. Odoevskii is the first major work on that significant figure in Russian intellectual and literary history since P. N. Sakulin's huge and fundamental contribution, *Iz istorii russkogo idealizma: Kniaz V. F. Odoevskii; Myslitel', Pisatel'* (1913). Sakulin's book, although it remains invaluable, is disorganized, incomplete, and in part out of date. The gap in scholarship must, at least to some extent, be related to the peculiarities of the subject, for Odoevskii, who for decades occupied a position near the center of Russian cultural life, repeatedly changed his many interests—and to a considerable extent his views—and failed to become either a great writer, like a number of his friends and acquaintances, or a recognized leader of an entire current of thought in the manner of A. S.

Khomiakov, V. G. Belinskii, or A. I. Herzen. Fragmentariness and variety could be considered the leitmotif of his life, and that is precisely how Cornwell presents him to the reader.

After Isaiah Berlin's foreword, a preface, and acknowledgements, the text is divided into a thirty-page biographical introduction—which is informative and sensitive while eschewing dogmatism and depth—and two parts. Part 1 contains chapters on Odoevskii as a writer, thinker, musician, and popular educator, notably his activities as pedagogue, philanthropist, and children's writer. Part 2 deals with Odoevskii's relations with tsarist society and with his cultural milieu, more especially his relationships with twelve individuals, including A. S. Griboedov, Pushkin, Gogol, Lermontov, Belinskii, Dostoevsky, Turgenev, and Tolstoy. The book also includes a brief conclusion, three appendixes, notes relegated to the back of the volume, a valuable bibliography as well as a selective index of works by Odoevskii, and a general index. Cornwell appears to have an exhaustive knowledge of everything published by and about Odoevskii, but he was allowed only a limited use of archives. The scholarly level of the book is very high. Indeed, mistakes are extremely scarce (although the authoritative formula of Official Nationality was and could only be "Orthodoxy, autocracy, and nationality," never "autocracy, Orthodoxy, and nationality," [pp. 179, 209]), and even mistranslations and misprints are few.

The book presupposes a fine knowledge of Russian cultural history and a deep concern with its detail. It often moves from vignette to vignette, each one more meaningful the more the reader knows on the subject, with repeated cross-references to other parts of the study. Although to state that the volume dissolves in detail would be unfair, for some larger conceptions do emerge, its life and strength certainly lie in the particular. Cornwell has chosen nominalism over realism. I would guess that many of the cognoscenti will find the volume a delight, although the more austere among them, as well as the less well informed readers, will consider the detail excessive and will desire more interpretation and guidance.

But, then, no book is perfect for all purposes. The author is to be thanked for a useful, important, and in certain ways excellent study of Vladimir Fedorovich Odoevskii.

NICHOLAS V. RIASANOVSKY  
University of California,  
Berkeley

FRIEDRICH DIESTELMEIER. *Soziale Angst: Konservative Reaktionen auf liberale Reformpolitik in Russland unter Alexander II (1855–1866)*. (Europäische Hochschulschriften, third series, Geschichte und

ihre Hilfswissenschaften, number 255.) Bern: Peter Lang. 1985. Pp. 451. 75 FR.

Friedrich Diestelmeier examines what he calls a "consciousness of crisis" or "crisis-consciousness" (*Krisenbewusstsein*) among the conservative Russian landed gentry during the decade of the Great Reforms (1855–66). Russian prerevolutionary historians tended to emphasize the achievements of the reforms (perhaps in the hope of encouraging further reform); those who came after the revolution, Russian and foreign alike, tended to see the period only through the prism of later events. In both cases, historians usually took for granted the conservative opposition without delving into its motives.

Diestelmeier proposes to fill the gap in the historiography by examining the widespread feeling of skepticism, pessimism, and fear of the future among the landed gentry in response to the transformations wrought by the reforms. His study is divided into three parts. The first is a short review of problems of research in political conservatism and sets the background for the rise of the "consciousness of crisis" among the gentry. When faced with the impending destruction of the institutions of serfdom on which its social, political, and economic status was based, the gentry, unprepared to attack government policies mainly (as others have shown) because of age-old habits engendered by its status as the main recipient of government patronage, reacted passively and ineffectively. This situation changed when Alexander II's show of commitment to the Emancipation Statutes provoked the conservative gentry to take concrete positions and participate in the growing public discussion of the reforms. These activities, though, put the gentry into opposition to the government and violated the principles of authority and loyalty to the tsar that the gentry associated with its own status and privileges.

The second and major part of the study (to which a short review cannot do justice) examines the most important features of the conservative gentry's "crisis-consciousness": its anxiety about status and its feelings of impotence and powerlessness in the face of new realities. Conservative landed gentry warned of an impending collapse of authority, economic ruin, and a loss of social identity brought on by social leveling. Overshadowing all other concerns was the threat, fueled by the historical memory of peasant revolts, of a coming revolution.

Diestelmeier's concluding section surveys the already well trodden ground of political activism in Russian society (which both appeared and then disappeared during this decade), with special emphasis on the remarkable degree of participation among conservative gentry. Ironically, by its very activism the gentry contributed to the dissolution of

the order whose conservation was its primary concern.

Although much of Diestelmeier's study is not new, it offers access to an array of interesting archival materials not yet touched on elsewhere. Beyond this, it is well written and based on what appears to be a nearly full review of published primary sources and secondary studies in French, German, English, and Russian. Although the history of the reform has been told before, Diestelmeier's examination of the conservative gentry will be of interest to historians of comparative political conservatism and of nineteenth-century Russia.

HEIDE WOELKER WHELAN  
Dartmouth College

REGINALD E. ZELNIK, editor. *A Radical Worker in Tsarist Russia: The Autobiography of Semën Ivanovich Kanatchikov*. Stanford: Stanford University Press. 1986. Pp. xxx, 472. Cloth \$45.00, paper \$14.95.

Russian labor history has benefited in the last decades from the efforts of energetic young historians (among them Victoria Bonnell, Laura Engelstein, Rose Glickman, Robert Johnson, Diane Koenker) who have established a solid monographic base for understanding the nascent working class that launched two revolutions and made possible the first "workers' state." Necessarily, the social historical investigations of Moscow and Saint Petersburg laborers have focused on aggregates of actors—their migration from village to city, the organization of study circles (*kruzhki*) and mutual-aid funds (*kassy*), unions and relations with Social Democratic activists—and inevitably some of the variety of workers' experiences was lost in the elucidation of general patterns. Reginald E. Zelnik, the author of a major study of Petersburg workers in the 1860s, has restored much of the texture of the workers' milieu in his superb translation of Semën Kanatchikov's memoirs. His protagonist, born in 1879 in a village west of Moscow, traces in simple and lucid prose his life's journey from peasant poverty through his rise as a skilled craftsman to his involvement in the illegal labor movement. The world he left behind in his father's village, seen from the postrevolutionary vantage of a worker-Bolshevik, was one of monotony and brutal despotism. At age sixteen he went to Moscow, where he found that both peasant religiosity and drunkenness survived in the factory. Before long he came under the influence of a fellow worker who poked fun at his fear of hell and opened up a new moral and intellectual world. Kanatchikov's shedding of Orthodoxy and acceptance of the need to remain sober and work well were part of his gradual rejection of his peasant identity (and, by



extension, of his father) and immersion into the formative culture of the workers.

From Moscow the skilled pattern maker made his way to "Piter," where he came into contact with students who hoped to radicalize the workers. At first Kanatchikov and his friends were wary of being entangled in politics, but by 1900 they were involved enough in the revolutionary movement to be arrested. From prison Kanatchikov was sent back to his village, but the psychic distance he had traveled from his forefathers made return to his old way of life impossible. He was already a "conscious" worker, and, in his later exile in Saratov, he became an active member of the local Social Democratic organization. The memoir ends in 1905, when experience and theory came together in the revolutionary moment that changed him from a worker-agitator into a professional revolutionary.

Students of Russian history and of labor history, as well as anyone interested in a "good read," will be grateful to Zelnik for his fluent and careful translation. One might regret the absence of an index, but the elaborate and fascinating endnotes answer any query that laypeople or specialists might have. This is a model text that allows us to hear a sensitive voice from the Russian past and come to our own understanding of how peasant-workers came to overturn the world that had made them.

RONALD GRIGOR SUNY  
University of Michigan

SILVANA MALLE. *The Economic Organization of War Communism, 1918–1921*. (Soviet and East European Studies.) New York: Cambridge University Press. 1985. Pp. xv, 548. \$49.50.

Silvana Malle's study of Bolshevik economic policies and institutions during the Russian Civil War is an important step toward understanding the foundations of the Soviet order. Like the valuable contributions of T. H. Rigby and E. G. Gimpelson and the work in progress of Lars Lih, this book has a specific focus but does not lose sight of broader questions. For Malle, these are twofold: the degree to which War Communism was shaped by socioeconomic context as opposed to ideology and the degree of correspondence between legislation and its implementation. These issues are explored as they relate to the nationalization of industry, money and the banking system, industrial management and administration, planning, procurement of food, military supply, and, more briefly, the militarization of labor.

Malle does not find singular answers to either of these broad questions, but this is hardly surprising. In some areas of economic policy and administration, such as the nationalization of industry and industrial management, she believes context loomed

larger than ideological prescription in shaping the Bolsheviks' response. In others, particularly the procurement of food and the regulation of trade, she sees the balance in favor of ideology. In each area, however, the matter was enormously complex, as Malle recognizes. One-man management (*edinonachalie*) was largely a response to the inefficiencies of collegial rule in the factories but was acceptable to many Bolsheviks ideologically inclined toward administrative centralization and the primacy of the party. Control over trade had strong theoretical foundations but emerged as policy in part because many believed strong measures had to be taken against speculation and black markets—the inevitable accompaniments to extreme shortages of food and commodities. The author's lack of strong conviction on these issues is the understandable result of carefully reviewing the evidence.

In dealing with these matters, Malle might have paid more attention to the differences between industrial branches and hence to the contradictions engendered by sectoral (and geographical) differences. For example, many Bolsheviks recognized even before 1917 that a decentralized, collegial administration was likely to be more efficient for the railroads than one-man rule, since the diverse aspects of line administration could not be well managed at the top. The opposite clearly held true for metal fabricating and textile plants, as well as the chemical industry. Part of the Bolsheviks' difficulty in structuring Civil War policy was their reluctance to tolerate diversity, itself the result of an ideological disposition as well as of political crisis. Also, the questions raised by Roy Medvedev and others about the baneful consequences of the trade monopoly might have been explored more fully here in terms of the actual extent of shortages of goods and the black market, although the problems of evidence are formidable. Further, Malle argues the contrast between policy and implementation much more descriptively in the case of food procurement than of industry, again no doubt because of limits in the sources. Yet a stronger line of inquiry in either of these areas probably would not have led Malle to different conclusions—strong testimony to the strength of her effort.

The author does not intend to press a particular viewpoint on these contentious matters or even to offer a concise definition of War Communism, which she prefers to regard as a period of time characterized by a particular set of institutions aiming at central control over production and distribution, rather than an explicit or prescriptive set of policies. Rather, she has attempted a straightforward description of Bolshevik economic policies and institutions in the period from 1918 to 1921 and has judiciously sifted a vast amount of difficult material.



She succeeds admirably, offering much of interest to students of Russia and others.

WILLIAM G. ROSENBERG  
*University of Michigan*

VITALY RAPOPORT and YURI ALEXEEV. *High Treason: Essays on the History of the Red Army, 1918–38*. Translated by BRUCE ADAMS. Edited by VLADIMIR G. TREML. Durham, N.C.: Duke University Press. 1985. Pp. xvii, 436. \$35.00.

This book has an unusual provenance. It was completed as a samizdat work in 1977 and then smuggled out of the Soviet Union. In 1980 one of the coauthors, Vitaly Rapoport, was allowed to leave and claim credit for his work. One cannot but be surprised either at the incompetence or at the lack of desire of the KGB to identify the pseudonymous Yuri Alexeev, the coauthor, who is here identified only as a Soviet general.

The circumstances in which the book was written, of course, leave their marks on the final product. Since the authors were obviously addressing their compatriots, they found it necessary to retell much of Soviet history. These pages hold only limited interest for the Western reader, who has access to much better and more reliable sources. A few early chapters on the Civil War, on Philip Mironov, and on the struggles for leadership of the 1920s can be safely skipped. Some of the authors' judgments are, to put it mildly, questionable. For example, few Western scholars would believe that in the mid-1930s the Red Army was the best in the world. Also, the Western reader might be put off on occasion by the authors' distinctly Soviet and heavy-handed irony. These are minor matters, however. We can all benefit from reading these essays. The book gives us new and interesting material, it allows us to see the mentality and world view of a presumably significant segment of the Soviet intelligentsia, and, at places, the authors raise large moral issues in a sensitive and moving way.

The most interesting chapters deal with purely military matters, especially the description of debates concerning strategy. The authors' hero is the former tsarist general A. A. Svechin, who cut through ideological nonsense and resisted political pressure in formulating his ideas. Svechin insisted on the importance of strategic defense and considered nothing sillier than the defense of every inch of native territory. Experience, of course, proved him correct. Naturally, Svechin had trouble with almost the entire military leadership, for whom only the idea of attack could be reconciled with revolutionary élan. Already in 1931 Svechin was removed from the Academy of the General Staff and imprisoned. He was killed in 1938.

Rapoport and Alexeev give us the most detailed description so far of the army purges of 1937. Here they provide us with new material: convincing and multidimensional character sketches of complex military figures and interesting anecdotes. The question immediately arises: since the authors could not provide us with conventional footnotes, how much can we trust them? Nothing that the authors say contradicts available evidence, and their version has an air of verisimilitude. Their speculations are sensible and are always labeled as such. Probably, we will never have a more reliable description of this dreadful episode.

The story they tell is, of course, horrendous. Of the top 899 military men, Stalin had 643 arrested; of these only 60 survived. (In an appendix the authors provide the names of those who perished.) Stalin succeeded not only in destroying the high command of the army but also in arranging that military men sat in judgment over their peers. Prominent leaders, such as V. I. Bliukher and Ia. I. Alksnis, became accomplices before they themselves became victims. Rapoport and Alexeev are correct in noting that after this slaughter the army had to be built anew. The Red Army as it had existed for the two decades after the Civil War was no more.

PETER KENEZ  
*University of California,  
Santa Cruz*

YURY BOSHYK, editor. *Ukraine during World War II: History and Its Aftermath; A Symposium*. Assisted by ROMAN WASCHUK and ANDRIY WYNNYCKYJ. (Canadian Library in Ukrainian Studies.) Edmonton: Canadian Institute of Ukrainian Studies, University of Alberta. 1986. Pp. xviii, 291.

The enormity (in all senses of the word) of the drama played out on the Ukrainian stage during 1939–45 has made it impossible thus far for any single work to provide more than a partial view. As the word "aftermath" in the subtitle hints, the focus of this symposium edited by Yury Boshyk is on recent accusations of criminal behavior among Ukrainians, and the authors' special concern is Canadian responses to these charges.

The first two essays, by Orest Subtelny and Bohdan Krawchenko, survey the Soviet and German occupations, and later chapters by Peter Potichnyj, Myroslav Yurkevich, and Mark Elliott directly examine collaboration. Although they present little new data, these scholars summarize most available information, including important evidence recently published in works such as *Litofys UPA*, nine volumes of documents on Ukrainian resistance. All the essayists recognize that some collaboration between organized Ukrainian forces and

Germans occurred and competently summarize the complex motivations. But most presentations tend to gloss over the impediments—whether to shrewdly self-interested collaboration or to effective resistance—imposed by naive adherence to integral nationalism and the moral implications for intra-Ukrainian relations of fanatic adherence to this creed. The documentary appendix (one-third of the volume) suggests how violent early prescriptions (the “Decalogue”) were and how bitter wartime experience altered this fanaticism. More novel are extensive excerpts from U.S., Canadian, and British official reports. Brusque orders forcibly to return displaced persons to Soviet control were replaced by humanitarian sympathy as officials gradually grasped the complexity of Eastern European events. Since such reports have constituted the basis for official policies since 1947, a review of the original arguments is salutary.

Part 2 (and Taras Hunczak’s essay in part 1 on Ukrainian-Jewish relations) comes to grips with current charges. David Matas, senior counsel of the League of Human Rights, B’nai B’rith Canada, presents a balanced case for the investigation to be pursued by the Canadian Commission of Inquiry on War Criminals. Matas insists that only individual guilt is at issue, for “collective guilt” charges against groups constitutes “an incitement to hatred.” Further, he advocates that the commission’s charge be extended to all wartime crimes against humanity, not only those committed by Nazis and their accomplices.

Ukrainian essays in part 2 generally accept these positions but concentrate on what Canada should do to avoid alleged errors of the Office of Special Investigations (OSI) of the U.S. Department of Justice. These errors include civil procedural forms, permitting prosecutorial appeal of court judgments favorable to the accused, and denial of public defenders’ services to indigent accused. As one might expect, strongly anticommunist Ukrainian authors are intensely suspicious of evidence provided by the Soviet regime. But, since the book’s publication, Robert Gillette, in lengthy articles in the *Los Angeles Times* (April 27–28, 1986), has endorsed the charges that evidence compiled by the KGB has been selectively supplied to the Justice Department. Possibly more broadly significant than these legal issues is the Ukrainian perception that American proponents of war-criminal charges are intent on extending them to Ukrainian organizations, if not to the ethnic group as a whole. A book by the former head of the OSI, Allan A. Ryan, Jr., (*Quiet Neighbors* [1984]), arouses ire because of his apparent deference to Soviet Prosecutor Roman Rudenko—who nourished his career aspirations through twenty-four years of service to Stalin in the Ukraine.

That pressing concerns of minorities have led to writing history with a special focus is not surprising. Ferreting out obscure septuagenarians, some of whose deeds certainly deserve public obloquy, may be the grim duty of prosecutors. But the common-law adversary process, especially with the limitations mentioned earlier, is hardly suitable for understanding the immense complexities historiography must confront.

JOHN A. ARMSTRONG,  
EMERITUS  
*University of Wisconsin*

RICHARD K. HERRMANN. *Perceptions and Behavior in Soviet Foreign Policy*. (Pitt Series in Policy and Institutional Studies.) Pittsburgh: University of Pittsburgh Press. 1985. Pp. xxi, 266. \$31.95.

Conflicting hypotheses about the motivations behind Soviet foreign policy have long bedeviled thoughtful analysts and, at times, even American policy makers and their advisers. Richard K. Herrmann first reviews the dominant schools of thought regarding Soviet behavior abroad, which he labels “communist expansionism,” “realpolitik expansionism,” and “realpolitik self-defense.” He finds that “it is disturbing that all three competing theories remain essentially nonfalsifiable and thus immune from empirical evaluation” (p. 20). Nonetheless, he proceeds to examine three “focused case studies” of Soviet behavior in 1967, 1972, and 1979, in the hope of finding answers by translating “the core-claims into more testable propositions.”

He discovers, however, that “the [communist] expansionist hypothesis . . . does not rest upon observable commitment and is impossible to test or to disprove” (p. 180). As for “realpolitik expansionism,” Herrmann finds that “we cannot conclusively determine whether . . . [there is] a lack of expansionist ambition or the lack of opportunity.” As for the third hypothesis, “although this study has not disproved the defensive hypothesis, it obviously cannot prove their accuracy either” (p. 192).

This is a very self-conscious and modest effort that includes a sensible critique of various methodologies and of some assumptions underlying policy advocacy in the United States. Herrmann is sound on the role of perceptions. He has read all (or almost all) the right books and articles. But, when all is said and done, his work, behind ample scaffolding, has problems of its own, many of them through no fault of his.

To give but one example, much of Herrmann’s argument rests on coding whether Soviet leaders and media at a given time described the United States as an “enemy” or as “degenerate.” Even if we were to accept the validity of a rather mechanical

(yet fundamentally subjective) coding and its quantified results, these two labels (and what they stand for) are scarcely exhaustive alternatives; their occurrence in the public media is of debatable significance for understanding Soviet decision makers' assumptions about the United States. The author also does not adequately consider the specific Soviet vocabulary of public pronouncements and the code underlying official rhetoric.

Historians will find this both an instructive and a frustrating example of the sophisticated search for better tools and better answers among young political scientists. It is a worthy attempt, even if the elaborate effort yields little that, to some observers, was not rather obvious in advance.

ALEXANDER DALLIN  
Stanford University

### NEAR EAST

REEVA S. SIMON. *Iraq between the Two World Wars: The Creation and Implementation of a Nationalist Ideology*. New York: Columbia University Press. 1986. Pp. xv, 233. \$30.00.

In the past ten years scholarship on Iraq between the wars has blossomed. Coming on the heels of pathbreaking studies by Hanna Batatu, Peter Sluglett, Mohammad Tarbush, and others, Reeva S. Simon's book is a disappointment.

To be fair, Simon's intent is originality of interpretation rather than of documentation. She sets out, as she explains in her introduction, to answer two questions: "Why did a group of army officers . . . wage a disastrously futile war against Great Britain [in 1941]? Why did these officers reject the British and liberal democratic values, turning instead to a militaristic Germany" (p. xi)? Going beyond the obvious explanation that Germany, lacking an imperial presence in the Middle East and being Britain's rival in Europe, was a natural counterpoise for Britain's nationalist opponents in Iraq, she adopts a thesis of cultural determinism: "[Iraq and] other developing countries such as Japan and Turkey found nonliberal European ideologies to be appropriate paradigms to emulate, because cultural nationalism and militarism were concepts compatible with their own cultural traditions" (pp. 167-68). According to Simon, this compatibility was discovered by Iraqis during their education at the military college in Istanbul, dominated between 1880 and World War I by German instructors, and was encouraged by German propaganda during the 1930s.

Simon's thesis of cultural determinism is profoundly ahistorical. From a moment in history—the period of the Third Reich—it extrapolates cultural

connections across time and space. It ignores the historically specific problems of nations coming late to nationalism in a world dominated by others and mistakenly deduces a uniformity among the varied cultures of the non-Western world, most of which at one time or another have rejected "British and liberal democratic values." It also exaggerates the difference between "liberal" French and British nationalism and the "nonliberal" German variety. Whereas she applauds the rationalism of Joseph-Ernest Renan's "Qu'est-ce qu'une nation?" which posits individual choice as the deciding factor in national affiliation, others have seen it as simply a clever argument in favor of France's claim to German-speaking Alsace. The distinction Renan made between "objective" Aryans and "subjective" Semites is part of the same intellectual tradition that spawned German racial theories.

To trace the rejection of liberal values specifically to German influence, therefore, is problematic. Certainly some Iraqis read German political philosophy, as well as English and French, and were educated by German staff officers, as well as by English and French. Some saw in the unification of Germany a historical situation more applicable to their own circumstances than French or British nation building. But, to conclude from this that German influence was responsible for the intervention of the Iraqi army in politics and the failure of an electoral system is a mistake.

Simon fails sufficiently to recognize that British liberal and democratic values were not in evidence in British policy toward Iraq or any other part of the Middle East. From Britain's occupation of Mosul in violation of the armistice of Mudros in 1918 to the thirty-days' war of 1941, Britain set an example in Iraq of "might makes right." Learning from this example, Iraq counted on its army to protect it from undue British interference and on Britain's enemies to help. The consequent politicization of the army and its intervention in politics did not necessarily mean that Iraq became the militaristic society characterized by Simon. Indeed, Iraqi society was far too diverse for the ideals of the military to penetrate very deeply.

Despite my fundamental reservations about her thesis, Simon's book is useful in parts. Her chapter on the development of national education in Iraq is original and especially interesting. Her chapters on the Sharifian officers and the Iraqi army are useful condensations of material in Tarbush and Batatu.

MARY C. WILSON  
New York University

### AFRICA

ROLAND OLIVER and G. N. SANDERSON, editors. *The Cambridge History of Africa*. Volume 6, *From*

1870–1905. New York: Cambridge University Press, 1985. Pp. xvi, 956. \$89.50.

This volume, written by several authors, carefully surveys the African continent at the time it suffered the intrusion of Europeans. The general and happy result of the writing is avoidance of the complaint registered by John Lonsdale in his concluding chapter that African history of this period has often been “smothered in high abstraction and wide generality” (p. 680).

As one has come to expect of work under the supervision of Roland Oliver and G. N. Sanderson, the essays in this general history are very well done because they have been very well assigned and edited. Accordingly, the contents are consistent in their analysis of political and economic factors, distinguished by their clarity of expression, and responsive to the latest considerations in scholarship.

The undergirding theme, if one exists, is that of European realization of “Africa as a whole”: observation and concern with the entire continent, not only with its regional or local elements. As Europe reinterpreted the world, it “discovered” Africa before most Africans did. This geopolitical discovery, sharpened by exploration of the interior of the continent and heightened by technological developments, made Africa, in the European imagination, a new place of political and economic concern.

As several authors point out, the penetration and partition of Africa were neither simple nor direct; rather, they were characterized by a series of forays, of fits and starts, before the Congress of Berlin at least pretended to map things out. As the interpretations in this book stress, no one politically alert in the decade before 1870 would have anticipated the events of the following three decades. Penetration seemed to proceed presentiment, with imperialist ideology trailing behind.

As must be so in a general survey, the familiar remains evident in comforting proportions. The basic lines of established African history are not sharply altered, but they are added to so that the resulting picture is both more detailed and more nuanced.

Each of the chapters upholds the high standards of scholarship that the series has established. The three most compelling essays, as might be anticipated in a general survey, are those of “overview.” A. E. Atmore’s “Africa on the Eve of Partition” is particularly refreshing in its economic interpretation and remarkable in its scope and analysis. Sanderson’s “The European Partition of Africa” and Lonsdale’s “The European Scramble and Conquest in African History” are surely among the best and clearest explanations of the causal pattern of the partition of Africa that have yet been offered. Both authors add a development that conditioned

the partition: Sanderson remarks that among the list of causes must be included the negative one: the collapse of British hegemony (p. 116); and Lonsdale asks “how far it was death (by famine and disease) rather than the Europeans which really conquered Africa” (p. 747).

Encyclopedic and analytical, this volume is a memorable addition to the general literature. The editors’ preface informs the reader of the interesting history of the volume itself. Its delay in publication has benefited every reader, for here indeed is an exceptional general survey in which new scholarly developments and old-fashioned style join to assure a most rewarding reading experience.

RAYMOND F. BETTS  
University of Kentucky

A. ADU BOAHEN, editor. *General History of Africa. Volume 7, Africa under Colonial Domination, 1880–1935.* (UNESCO General History of Africa.) Berkeley and Los Angeles: University of California Press, 1985. Pp. xxvii, 865.

Some twenty years ago, UNESCO decided to appoint a commission to write the history of Africa from earliest times to the present. This is the next to last volume of a lavish, multiauthored work, filled with informative photographs, maps, tables, and bibliographies. Although intending to produce a scholarly work, the UNESCO commission also sought to bring forth a book that would be “a vitally important element in the recognition of the African heritage and [that] should bring out the factors making for unity in the continent” (pp. xxv–xxvi). Although not all periods can easily be seen as reflecting African unity, certainly the colonial period, which this volume covers, on the whole provided a common African experience. Except for Ethiopia and Liberia all of Africa fell under European control and then lived under alien white rule.

Reading the many chapters, I find it difficult to know when each was written; they seem at times to be anachronistic. If written in the early 1960s, the complaint that the writing of African history is Eurocentric seems appropriate, but, for a volume that has come into print in 1985, the comment no longer seems relevant to African history writing. Although the chapters are competent and worthy syntheses of scholarship published in the last quarter century, many of them in a self-congratulatory style claim to be pioneering work. Much important historical work of the last twenty years has been clearly Afrocentric and has eschewed European categories of thought.

The travails afflicting A. Adu Boahen in editing this volume are obvious when one notes that, prior to its appearance, two authors of chapters had died

and four chapters were rewritten by someone other than the original contributor. Why this was necessary and in what manner the original contributions were transformed are left unexplained. Some of the initial authors are international authorities, and one would be curious to know how their contributions were unsatisfactory, needing to be redrafted. For a work that proudly proclaims its Afrocentrism, the book curiously reflects a number of Eurocentric atavisms, among them its tendency to categorize individuals who were products of Western schools as "educated Africans." Obviously, Africans have always been educated in the sense that they have been exposed to the education that their society deemed proper for them.

In part intended to serve independent Africa, this volume concentrates on resistance to colonial rule, the rise and development of various protest and nationalist movements. One of the best chapters is Terence Ranger's masterly discussion of the various dimensions of resistance to European conquest. This chapter is followed by numerous chapters dealing with resistance in various regions of Africa. European methods of rule, the nature of the colonial economy and its impact on each region, political developments, and the stirring of nationalism are also examined.

The chapters show a variety of interpretations; the editing allowed for a pluralism of views that is laudatory but also confusing. Were the loans that the khedive of Egypt took "thrust" on him, as one author avers, or rather the result of poor management and irresponsibility, as another author argues? Were African states the victims of European imperialism or rather themselves actors in making European conquest possible by collaborating with European powers to advance their own interests? Both arguments are advanced as overall generalizations. The authors' values vary considerably; some are Marxists, some are not. This variety adds to our understanding of the differing ways in which African history may be viewed. But I must say that I was puzzled by the statement that foreigners in Egypt enriched themselves by "immoral means." Without further details, one is left wondering what this immorality entailed and what is the author's scale of values.

Unusual for a history of Africa is the inclusion of a final chapter dealing with the relationship of blacks in the United States and Latin America with the continent of their ancestors. They at times supported and encouraged African self-help and protest movements and played some part in the eventual winning of African independence. Nevertheless, the inclusion of this chapter appears more important as an assertion of a pan-African ideology that includes the black diaspora than as a significant addition to our knowledge of Africa.

In an eight-hundred-page book covering a continent over two generations, written by nearly three dozen authors from two dozen countries, one can find a lot of unevenness in the quality of research and writing. But one has to salute the prodigious effort that has gone into producing this monumental work, which has already appeared in English, Arabic, and French and later will also be produced in Swahili and Hausa. The intention of the UNESCO committee is to see the history appear in Chinese, Portuguese, Russian, German, Italian, Spanish, and Japanese.

WILLIAM B. COHEN  
*Indiana University*

LUCY E. CREEVEY, editor, *Women Farmers in Africa: Rural Development in Mali and the Sahel*. (Papers Presented at the Bamako Workshop on Training and Animation of Rural Women, 1983; Contemporary Issues in the Middle East.) Syracuse, N.Y.: Syracuse University Press, 1986. Pp. xvi, 212. Cloth \$29.95, paper \$14.95.

*Women Farmers in Africa* presents papers given at a Food Corps Programs International workshop. Lucy E. Creevey edited the volume. Contributors include several American and European experts on development, but the majority are African women involved in implementing development projects in the Sahel—in Mali and elsewhere. The contributions of the African women should have lent the book interest and strength; little reaches European and American audiences directly from Sahelian women. But the high potential for adding to the meager store of our knowledge on Sahelian rural women is somewhat frustrated by reality.

The book has good aspects; a solid faith in development from below is demonstrated throughout in various forms. Most contributors emphasize the necessity for local initiative, energy, and input in the instigation and implementation of development projects. Otherwise, projects will fail. Experience verifies this assertion, as well as the disastrous effects of ignoring women, who produce most of Africa's food and have critical input in producing cash crops as well. Another strength shown is some necessary rethinking of the sexual division of labor in Sahelian areas that points out an even greater importance for women's labor than has been accorded to it previously. This is, however, almost the full extent of useful information in the book.

Creevey stresses the need for hard data concerning results of implementing various strategies of development and therefore the value of having contributors who are not scholars. But the participants in the project mostly restrict their observations to generalizations rather than documentation. Some



papers are marred by a lack of faith in the rural women whom they view as a source of regenerative development. For instance, Creevey herself attributes problems with rural women adopting innovations to social customs, an old view pioneered by missionaries in Africa (p. 10). Logically enough, she then does not remedy some further examples of old thinking in her contributors, who occasionally use such words as "tribe" and "fetishist," which have negative connotations for many Africanists. In chronicling the failure of a cooperative, Creevey (p. 101) blames rural women's "poor understanding" of its objectives, rather than the fact that most members received nothing from it and so abandoned it—surely a rational act for those who cannot afford to devote their limited and overextended resources to an unprofitable enterprise. In more thoughtful analyses represented by Kate Cloud and Jonathan Tucker in this volume, economics plays a stronger role in determining the success of a project.

The book illustrates some of the limits to the accomplishment of development from below by rural women but is also limited itself by resolutely adhering to a local view. Creevey ably and interestingly provides unity by introducing each contribution. However, after pointing out the necessity for local input and a multifaceted approach to generating income for women and some of the unintended negative effects of projects, she does not (nor does anyone else) rise above the level of the project to question the sometimes arbitrary acts of the governments of countries that donate or receive aid. Instead, she classifies them (in unintentional irony) with natural disasters as uncontrollable factors. For, no matter how hard rural African women work—and they work very hard indeed—they can instigate only limited changes. Only governments can implement substantial timely change on a large enough scale. We can only hope that grassroots input and control is perceived as necessary before complete disaster overtakes the Sahel. This book is an attempt to publicize that need; unfortunately, its execution is flawed.

CLAIRE ROBERTSON  
Ohio State University

DAVID ROBINSON. *The Holy War of Umar Tal: The Western Sudan in the Mid-Nineteenth Century*. (Oxford Studies in African Affairs.) New York: Clarendon Press of Oxford University Press. 1985. Pp. xv, 434. \$55.00.

David Robinson begins his book by putting several hostages to fortune dangerously at risk. He states, for instance, that "no scholars have worked across the full range of documentation, including the abundant oral tradition, and none have provided a

full and plausible interpretation" (p. 5) and later that "the most consistent failing in the secondary literature on Umar . . . is an inability to understand Futa Toro" (p. 66). Space allows only the briefest comment on such reckless exposures. First, Robinson's own coverage of the documentation is conspicuously inadequate in that he fails to analyze Umar's theological writings, especially the *Rimāh*, to say nothing of those of his disciple, Muhammad al-Rājī. Indeed, neither Umar's important *Suyūf al-sa'īd* nor Rājī's *Risāla* are mentioned in the text or in the bibliography, as far as I could discover. These serious omissions should have made Robinson wary of throwing down gauntlets. For their consequence is that a central aspect of the jihad—its intellectual history—is ignored. To observe, as on page 195, that Tijāniyya teaching had virtually disappeared (by ca. 1856) is of little use if one is never told what that teaching was and what its lapidary function may have been. And when he sets out to repair earlier failures to understand Futa Toro, by which one assumes he refers to the complexity of the tribal and clan structure of its society, he does so with a tangle of detail whose point and purpose are difficult to distinguish.

Fortunately, the book in general displays clarity and breaks some new ground. For instance, Robinson's thorough examination of Umar's relationship with the French makes clear, first, that the jihadist had every initial intention of profiting from a friendly French connection and, second, that he was forced into hostilities with them by circumstances that were more compelling than the wish on each side—both Umar's and Louis-Léon-César Faidherbe's—to avoid such hostilities. The "resistance model" is thus—finally, one hopes—laid to rest.

Robinson's other central argument, that the Umarian jihad was an ethnic movement of Fulbe imperialism aimed at conquest, not internal revolution, is well argued. When he seeks to place this in stark contrast to the Uthmanian jihad, however, he presses the point too far. The differences between the two movements seem to have resulted more from the direction circumstances imposed on each of them than from basically different originary concepts.

Two points struck me as intellectually refreshing in Robinson's narrative. First, he gives prominence to the personality of Faidherbe. In contrast to an earlier postcolonial tradition of West African historical writing that sought to adopt an African-centered stance to the extent of virtually denying any significance to the European presence (except to excoriate it!), Robinson recognizes Faidherbe as a central arbitrator in the outcome of the jihad and gives this outstanding soldier and administrator full credit for his intellectual gifts and sensitive handling of the problems that confronted him.

Second, Robinson pays due attention to the purely military factor, which many historians of West Africa prefer to ignore. His section, "Europeans, Weapons and Casualties" (p. 329), with its discussion of the double-barrelled musket in the cavalry role, the use of cavalry for pursuit and consolidation (which Umar enjoyed but the French initially lacked) and the influence of shrapnel on the outcome of the infantry battle, is a pleasure to read.

Much of what Robinson has to say seems to me to adumbrate a central fact of the Islamic experience in the western Sudan: the constant, idealistic drive for Islamic "reform" must have the equal and opposite effect of sparking off reaction against it, on the part not so much of pagans but rather of "mixers," those natural empiricists who adhere to Islam partially for its conveniences but balk at being pushed too far. The resulting frustration gives rise to cyclical violence between reformers and mixers. This is termed jihad because it occurs within an Islamic frame of reference, but such violence is in fact inherently structured within western Sudanese society as a consequence of the introduction of an ideology that, in its turn, is an aspect of the introduction of literacy. The cycle will only be broken when some overriding authority—for example a colonial administration or secular military government—intervenes to do so. Robinson's reference to the distinction Umar made between "simple pagans" on the one hand and "betrayers," "accursed," and "enemies of God" on the other is one example among many that could be quoted to support the validity of this interpretation (p. 351).

The maps are copious and excellent. I fail to see the usefulness of several of the tables. Despite criticism of detail, I welcome the work as a scholarly one that provides some new insights and certainly provokes thought.

MERVYN HISKETT,  
EMERITUS  
*University of Sokoto*

ROY ARTHUR GLASGOW. *Nzinga: Resistência africana à investida do colonialismo português em Angola, 1582-1663*. (Debates, number 178.) São Paulo: Perspectiva. 1982. Pp. 198.

The famous African queen Nzinga alternately appeared to collaborate with seventeenth-century Christian missionaries and Portuguese governors in Lisbon's colony of Angola and to battle doggedly against the Europeans' territorial conquests and meddling in the politics of the Mbundu states of that region. Although data on her personality rank high among the documentation on the obscure African political figures of that early period, her motivations and her complex political strategies remain hotly

debated. The highly historicist strain of interpretation represented in this semipopular biography portrays her as the prototypical heroine of African resistance to foreign domination and as guerrilla fighter extraordinaire.

Roy Arthur Glasgow's hagiography reflects a political image of Nzinga developed in the late 1960s by nationalists of the Popular Movement for the Liberation of Angola, who in 1975-76 became the ruling party in independent Angola. But the book does not qualify as critical history. It lacks the most rudimentary sense of the historiographical context of its subject, and the author indiscriminately mixes primary, secondary, tertiary, and even semifictionalized materials as sources. A limited random check of footnotes revealed none bearing on the points they were cited to support. Errors of detail fill the bibliography, and multiple errors of technique and fact mar the text. This Brazilian edition derives from an English manuscript written in about 1970 and incorporates nothing of the substantial scholarship since then on early seventeenth-century Angola. Some of the maps are utter nonsense. The translators have made naive and inaccurate conversions of what seem to have been the author's English renditions of the original Portuguese terms and sources. Why a volume bearing a publication date of 1982 should have reached the *AHR* only in 1986 is not clear. Five pages of the conclusion and a short documentary appendix appeared totally blank in my copy of the book. Knowing for certain what they should contain would not be likely to alter my judgment that students and colleagues should resolutely steer away from the unmarked shoals hidden beneath the scholarly surface of this book, unless they are knowledgeably sailing the shallower waters of political ideology in modern Angola and Brazil.

JOSEPH C. MILLER  
*University of Virginia*

KEVIN SHILLINGTON. *The Colonisation of the Southern Tswana, 1870-1900*. Johannesburg: Raven; distributed by Ohio University Press, Athens. 1985. Pp. xxi, 311. \$20.95.

Kevin Shillington's study fills an important gap in South African historiography. The southern Tswana (used here to denote the Tlhaping, Rolong, and Tlalo chieftaincies of the crown colonies of Griqualand West, 1871-80, and British Bechuanaland, 1885-95, later annexed to the Cape Colony as the northern Cape) have been largely ignored by historians. Yet these people were the first Africans caught up in the South African mineral revolution. The diamond discoveries of 1869 in their region drew the southern Tswana inexorably into the eco-

nomic and political changes of the period. This is a story of innovative response in increasingly onerous conditions. Initially, the diamond discoveries provided opportunities for the southern Tswana, who played an important role in the early prospecting and trading of diamonds in the region. Local Africans discovered most of the early diamonds in the Vaal River area. By late 1869 they were trading diamonds for horses, cows, cash, and other items, and local chiefs were levying fees for digging rights.

Although the Cape government soon brought these activities under control, the southern Tswana continued to profit from other aspects of the diamond trade. The Tlhaping people in particular prospered by providing provisions, especially food and firewood, to the expanding market in the diamond center at Kimberley. The strength of this rural economy is "illustrated by the fact that relatively few Tlhaping men entered into labour contracts at the Kimberley mines until 1876" (p. 65). This prosperity extended to British Bechuanaland as well.

But after 1880 the story changes. Disasters such as cattle disease, overcropping, overgrazing, excessive deforestation, and population expansion weakened the economy. But Shillington amply proves that the most deleterious impact came from colonial policies developed to assist capitalist farmers and mine owners in acquiring cheap labor and protected agricultural markets. Much of the value of this book lies in its detailed description of colonial authorities' often haphazard response to pressures from white farmers and mineowners. It is a depressing saga of state support for cattle stealing, land grabbing, and other forms of theft. Colonial solutions, such as land reserves, closed compounds, and co-optation of native authorities, took shape during this process and provided a model for other parts of the colony.

The book has a few shortcomings. The details overwhelm all but the most determined specialist, making arguments hard to follow. The congruence of Afrikaner and British South African interests is described but not highlighted. Differences among competing factions of the ruling class are underplayed, creating an unwarranted impression of state solidarity. But, overall, the book contributes much to our understanding of South Africa in the early period after the mineral revolution.

JANE L. PARPART  
Dalhousie University

#### ASIA AND THE EAST

BENJAMIN I. SCHWARTZ. *The World of Thought in Ancient China*. Cambridge: Belknap Press of Harvard University Press. 1985. Pp. 490.

This study is obviously the product of many years of research, thought, and reflection. The range of topics and issues involved would deter most scholars from undertaking anything more than a general treatment of the subject or a specific, topically focused study. Benjamin I. Schwartz's work, on the contrary, is an integrated, in-depth analysis of the tenets of the major philosophies of ancient China within the context of what may have been an intellectual world of highly interactive philosophical discourse. He brings to bear a thorough immersion in the Chinese texts and the secondary literature on the subject, as well as an extensive knowledge of the religions and philosophies of other civilizations of the "axial age" and of modern Western social scientific theory. He gives careful attention to the texts as a whole, including "motifs" and passages that previous scholars have ignored, dismissed, discounted, or rejected, and compares their contents to the philosophies of other contemporaneous cultures, especially ancient Western thought, with an occasional excursion into relevant Western social scientific theories. The result is a great many interpretations differing with points expressed in most, if not all, previous studies.

Schwartz's argumentation and interpretations are very persuasive. His differing interpretations on specific philosophic points are not, however, the only important contributions of the study. His analysis reveals a vibrant intellectual discourse that responded to changes in the age and quite possibly to the imperatives of the discourse itself. The diversity and divergence of that discourse unfold throughout the narrative. But in our earlier division of this thought into formally defined schools and sharp emphasis on their differences, we have obscured the shared cultural assumptions stressed throughout Schwartz's study. His more balanced discussion of shared assumptions and differences provides a clearer picture of the dynamics of the intellectual history of the period and may also lead to a better understanding of the process of unification in the Ch'in-Han and the rise of Confucianism. Finally, underpinning his use of comparative data is the conviction that a "kind of universal human discourse" (p. 14) is possible and a faith that "comparative thought, reaching across the barriers of language, history, culture, and Foucault's 'discourse' is possible" (p. 13). This faith, he concludes, "assumes a common world of human experience" (p. 13). Despite reservations about the value of cross-cultural comparative studies, the provocative methodology of this study should be taken seriously by all historians of China and other civilizations as well.

Schwartz's interpretations are not beyond disagreement. I suspect that his book will trigger a great deal of debate and discussion among students of Chinese thought. The discussion should engage

the attention of students of other histories as well. Clearly, however, this book is an extremely important and valuable study that will long serve as the basis for this discussion and subsequent studies; it constitutes a contribution that must be seriously considered by all historians.

ROBERT B. CRAWFORD  
*University of Illinois*

PRISCILLA CHING CHUNG. *Palace Women in the Northern Sung, 960–1126*. (T'oung Pao, Revue Internationale de Sinologie, number 12.) Leiden: E. J. Brill. 1981. Pp. xx, 129. f. 56.

The Chinese imperial household establishment in many ways resembled an elite Chinese family writ large. In an elite family most of the women were maids, who served perhaps a dozen years before being married out. Higher ranking and generally less numerous were concubines, mates of male members of the family. Each adult man also had one wife, whose status depended on his (the wife of the father outranked the wife of the son, and so on). Widows had more authority than wives with husbands, and the widowed mother of the senior male in the family often had her way in almost all affairs.

In the imperial household of the Northern Sung (960–1126) studied by Priscilla Ching Chung two to three thousand women were employed as servants, taking care of clothes, food, cleaning, and so on. In later dynasties, at least, such women left to be married by about twenty-five. Unlike in elite households only one married man lived in the palace; the emperor's sons and brothers were sent elsewhere. The mates of the emperor included a half-dozen or more who held titles as consorts and one empress. In addition there were the mates of previous emperors. The last emperor's empress was empress dowager; the mother of the current emperor, if different, might receive that title if the empress dowager died. Empress dowagers could exert considerable influence over emperors (even if they did not bear them themselves), and if a son succeeded to the throne as a child the empress dowager acted as regent, making political decisions normally left to the emperor.

In this study Chung analyzes the power available to palace women. She describes the administrative hierarchy of the service organization with its titles and ranks modeled on the civil service. She sees palace service as an alternative occupation for women, since in a few cases women of good family took employment there and women sometimes moved from the service organization to become consorts of the emperor (just as maids were sometimes promoted to concubines in ordinary families). Much more is known of the consorts and empresses, since ninety-two of them are subjects of brief biog-

raphies in historical sources. From these biographies Chung analyzes their family background, their method of entry into the palace, the ways their family benefited from their placement, and their political influence if they became imperial favorites or regents.

As a corrective to the view of the Chinese harem as a place of sexual and political intrigue, Chung emphasizes the institutionalized power of palace women. She lays great stress on promotions in rank, even ones received posthumously. Since historians pay little attention to the posthumous titles given male officials, I am puzzled by this. Did posthumous honors matter more to women than to men? Also, one may question whether the benefits a family received should always be interpreted as evidence of a woman's power. A family that sold a daughter to become the concubine of a rich merchant profited from her placement, but was this a sign of her power? Did a woman whose brothers attracted the attention of the local elite after she entered the palace feel powerful or used? Chung herself points out that some consorts preferred to keep their families at a distance.

Regents, of course, exerted political power comparable to that of men, but only two regents were of much consequence in the history of the Northern Sung. Other empresses and consorts influenced politics in noninstitutionalized ways, through their personal relations with their husbands and sons. In this they were similar to wives and mothers in other Chinese families.

The greatest contributions of this book to the history of women in China are its systematic mining of the information available in the biographies of empresses and consorts and its description of the administrative structure of palace service. Using this as a basis, future historians can explore changes in the political role of women and their relation to changes in ideology and social structure.

PATRICIA EBREY  
*University of Illinois*

JACQUES GERNET. *China and the Christian Impact: A Conflict of Cultures*. Translated by JANET LLOYD. New York: Cambridge University Press or Maison des Sciences de l'Homme, Paris. 1985. Pp. vi, 310. \$49.50.

Originally published in French in 1982, this analysis of the failure of the earliest sustained encounter between Chinese and Western civilizations is an impressive exploration of different cultural points of view. Jacques Gernet has produced a masterly work true to the Chinese past he has studied with such eloquence and to the Western European heritage he also honors by significant scholarship.



Whereas other studies of the Jesuits in China focus on their failure to convert the Chinese despite a strategic appeal to China's literate elite, this work concentrates on the impossibility of Christian-Chinese intellectual compatibility in the seventeenth century. The Jesuits were up against a different language, "different mental categories and modes of thought" (p. 3), and "a different kind of humanity" (p. 247) that "constituted a considerable obstacle to Christianisation" (p. 246).

Gernet approaches the cultural differences that surfaced in connection with the Jesuit mission in China through a study of the representation of Christianity in three sets of writings: first, the missionaries' works in Chinese; second, missionary writings in Italian, Latin, and French, including critiques of the early Jesuits' accommodative approach to the Chinese; third, contemporary writings by Chinese converts and critics in the seventeenth century. Gernet gives special attention to Matteo Ricci's *Tianzhu shiyi* ("The True Meaning of [the Doctrine of] the Master of Heaven," definitive text 1601), Yang Guangxian's *Budeyi* ("I cannot contain myself" or "I must burst out at last"), and the anti-Christian collections *Poxie ji* ("Collection for the Destruction of Vicious Doctrines") and *Pixie ji*, which were introduced by Paul Cohen in his pioneer study of Chinese religious antiforeignism, *China and Christianity: The Missionary Movement and the Growth of Antiforeignism, 1860-1870* (1963). Gernet's selection of readings is revealing, for it shows what the missionaries seemed to say, what they thought they were saying, the impact of their message on Chinese who represented and set cultural assumptions, and what aspects of Christianity Chinese writers propelled and perpetuated in the Chinese idiom.

Ricci was the first Christian missionary to establish a working mission in the Chinese empire and was well inside China by 1583. He died in Peking in 1610, following a distinguished and rather successful effort to impress Chinese scholars of the validity of Western learning—mainly through science and technology—and thus to interest them in the ideas of the Christian universe. Ricci's reports to the church show a deliberate strategy of studying Chinese classics in search of affinities that would give credence to the missionary message of God and salvation. It was, in Gernet's phrase, an "enterprise of seduction" (see chap. 1) designed to appeal to the pacesetters of Chinese society. The strategy was effective, for Ricci and his colleagues hobnobbed with officials and converted several men of letters, including Yang Tingyun (1557-1627), who was baptized Michael in 1612.

As Chinese administrators and men of letters came to be more familiar with the Jesuits and their writings in Chinese, they become suspicious of the accommodative relationship with the Western

world, for the Christian message threatened Chinese perceptions of morality, order, and proper behavior. Ironically, the early Jesuit strategy provided the means for a Chinese critique, for the Jesuits themselves introduced the Chinese terms for what the Chinese came to regard as heterodoxy. The central problem with Christianity, from a Chinese point of view, was its separation of the rational and the senses, the spiritual and the material. Gernet's analysis of the Chinese critique and splendid summary representations of the Chinese intellectual tradition add up to coherency that rejects duality: "The Chinese thesis is that there is no meaning outside the world and that body and spirit cannot be separated" (p. 197). Whereas the Jesuits maintained "that the principle of morality lies in God," before whom all creatures are equal, "Chinese morality consists in a continuous process of perfecting the self" (p. 56). In the Chinese universe heaven may insure the perpetuation of earthly order (p. 61), but humanity is the source of morality. Gernet asserts that "in trying to assimilate the Chinese Heaven and the Sovereign on High to the God of the Bible, the Jesuits were attempting to bring together concepts which were irreconcilable" (p. 193).

The missionaries who followed Ricci's method simply failed to understand fundamental cultural patterns that prevented Chinese acceptance of the Christian universe. Chinese men of letters at first listened to the missionaries, appreciated what appeared to be their respect for China's intellectual foundations, learned from their criticisms of Buddhism, and then put down Christianity. As Yang Guangxian observed, if the Christian doctrine allowed forgiveness of the crimes of the repentant such that they might ascend to paradise, then "in all conscience, their paradise must be a proper den of thieves" (p. 169).

Gernet thus perceives Chinese resistance to Christianity as more than mere xenophobia. Although one may fault Gernet for too neatly drawing Chinese and Christian civilizations as two different sets of assumptions, one likely can find no work more instructive of the Jesuit experience in China. This is an important, well-written, and absorbing book (even though one might wish for Chinese characters, at least in the bibliography, as well as for better editorial consistency and the absence of typographical errors); its lessons in cultural misunderstanding extend well beyond the Jesuits and the seventeenth century.

SUZANNE WILSON BARNETT  
University of Puget Sound

CARL T. SMITH. *Chinese Christians: Elites, Middlemen, and the Church in Hong Kong*. New York: Oxford University Press. 1985. Pp. xvii, 252. \$25.00.



This volume, a collection of articles on Hong Kong local history by Carl T. Smith, was published to mark the twenty-fifth anniversary of the twentieth-century version of the Hong Kong Branch of the Royal Asiatic Society. Smith has been, since his arrival in Hong Kong in 1961 as a teacher of theology, an avid local historian and genealogist. The ten chapters of the book were published in various Hong Kong publications over a period of almost twenty years.

Only three of the essays deal directly with the church. The other seven are concerned with the emerging identity and life patterns of the Chinese elite of Hong Kong. Many of them were Christians, but more important was their rise from obscurity to influence and wealth as contractors, merchants, compradors, professionals, and government employees through the vehicle of an English-language education. Interestingly, though products of a society marginal (and quite foreign) to China, some of them and their children entered Chinese government service in the late nineteenth and early twentieth century.

This volume has some definite drawbacks. Some of the articles are too old to be properly grounded in relevant, China scholarship. The contents are repetitious. Moreover, the three chapters on the church are too broad and short to be more than superficial.

Yet I think that many scholars of China will find this book quite useful. Smith's love for early and mid-nineteenth-century local history is evident. He has made diligent use of local and coastal newspapers and Hong Kong land registry and legal records. He has also mined the American Board archives at Harvard, the London Missionary Society archives, and Colonial Office records, as well as German-language materials of the Basel mission. Chinese characters for persons are included, when known, and the multiple names and aliases of several important figures are painstakingly sorted out. A fascinating appendix lists all known Chinese Protestant converts in Southeast Asia and on the China coast from 1813 to 1843. The index is quite complete for names. Historians of China will find rare bits of information here on, among other things, relatives of the Taiping leaders, Sun Yat-sen and his in-laws, the diplomat Wu Ting-fang, and several other important figures.

I recommend this volume, especially in light of the difficulty of access in the United States to the original Hong Kong periodicals.

DANIEL H. BAYS  
*University of Kansas*

DAVID JOHNSON *et al.*, editors. *Popular Culture in Late Imperial China*. (Studies on China, number 4.)

Berkeley and Los Angeles: University of California Press. 1985. Pp. xvii, 449. \$45.00.

The great value of this conference volume, edited by David Johnson, Andrew Nathan, and Evelyn Rawski, lies less in the specific information it imparts about popular culture in late imperial China (which although interesting is necessarily selective) than in its exploration of the problems involved in doing research in this treacherous area and in its limning of strategies for grappling with these problems. Western anthropologists, of course, have been studying Chinese popular culture for decades. But most of their efforts have been based on field work in twentieth-century Chinese communities, with relatively little reliance on written materials. Owing to historical circumstances, moreover, much of this anthropological work has been focused on Taiwan and Hong Kong, which cannot be assumed to be representative of the rest of China. Historians, in short, can learn a lot from anthropologists (as the pages of this volume amply attest), but the evidence with which we must work is generally quite different in nature, and, therefore, our strategies for research must also be different.

Although always a little risky, it is possible in certain circumstances to "read back" from the observable data of the present to the unobservable past. This is done in varying degree by three of the contributors to the book under review: James Hayes, in his informative study of the importance of specialists and written materials in the village world, Barbara Ward, in her essay on the role of regional opera performances in the dissemination of Chinese culture and values to ordinary people, and James Watson, in his fascinating account of the evolution of T'ien Hou as a popular goddess along the South China coast from the tenth century to the present. Each of these authors, either by supplementing field work with written historical data or by carefully tailoring their historical inferences to the specific nature of the data their field work has generated, is able to make a convincing statement about the popular culture of pre-twentieth-century China.

The other contributors to the volume, most of whom use either literature (in the broadest sense) or religion to examine Chinese popular culture, rely more exclusively on written materials. Some of them, most conspicuously Susan Naquin in her superb account of the evolution of the White Lotus sectarian tradition, take us directly into the realm of popular culture. Others attempt to reconstruct the values to which the population at large was exposed by in-depth analysis of literature written by elites for a popular audience. One problem encountered in these latter contributions is the difficulty in determining with confidence exactly who was "reached." A second difficulty, which Rawski underscores in

her compact but illuminating concluding chapter, is that even when we know who was reached we cannot be sure exactly what they were reached with: participants in a culture may not seek "to understand everything they see or hear" (p. 401). The potential gap noted by Leo Lee and Nathan between what people actually liked or believed and what members of the elite wanted them to like or believe did not just emerge with the rise of "mass culture" beginning in the late Ch'ing (the subject of Lee's and Nathan's contribution). It appears to have had a long prior history as well.

Although, inevitably, some of the contributions to the book are less useful than others, the volume as a whole must be reckoned an impressive success. It opens up vast stretches of new territory that historians have scarcely touched, and it is fairly bursting with helpful suggestions for the exploration and mapping of this territory. Ph.D. candidates in search of thesis topics will have a field day.

PAUL A. COHEN  
Wellesley College

ANNE WALTHALL. *Social Protest and Popular Culture in Eighteenth-Century Japan*. (Association for Asian Studies Monographs, number 43.) Tucson: University of Arizona Press. 1986. Pp. xviii, 268. \$19.50.

Peasant protest in Tokugawa Japan has recently attracted considerable scholarly attention. Anne Walthall's study is distinctive and significant for two reasons. It centers particularly on popular political culture and how that may be read from, and into, commoner protest. Moreover, she has focused on a single decade, the 1780s, perhaps the most critical ten-year span in the Tokugawa period, during which state authority was shaken badly by corruption and fiscal insolvency but then restored to a modicum of viability through the reform initiatives of Matsudaira Sadanobu. Walthall interprets the frequent protests of the decade to reveal "the commoners' ability to make their own world within the Tokugawa order" whose premises they aggressively challenged (p. 102).

The opening chapters usefully sketch the Tokugawa state system and the modal pattern of collective protest, the *ikki*, which drew on pre-Tokugawa traditions of egalitarian solidarity and which fully exploited and frequently exceeded permissible paths of grievance and remonstrance. The first resort was legal petitioning, and chapters 3 and 4 treat its many forms and formulas. Typically, in confrontations with officials, a conventional language of dearth and desperation framed pleas for protection, whereas, in challenges to commercial barriers and monopolies, petitioning agitated for the reverse, for free and open access to marketing.

Walthall details a number of cases to show how commoners creatively exploited and stretched the stylized syntax and constrained logic of the petitionary format. They made selective uses of the past as precedent, of other regions as referent, and of "common sense" to justify their own, often contradictory, definitions of self-interest.

Chapters 5 and 6 shift to the more direct, occasionally violent actions of the decade. Within rural villages these were often attacks on headmen, whose official conduct and prerogatives were increasingly challenged by commercialization of agrarian social relations. In the towns the decade saw a number of mass attacks on the property of wealthy merchants suspected of price rigging and market control. Walthall here again emphasizes her theme of an autonomous commoners' political culture. By the 1780s the crowds appealed less to Confucian virtues of paternalist benevolence and more for the divine retribution of avenging popular deities.

The final two chapters take up a neglected but pervasive characteristic of Tokugawa protest: the retrospective fashioning of written and oral accounts by which uprisings were remembered and reexperienced. Walthall contrasts "chronicles," assembled from documents and reports by local officials and others concerned about protecting reputations and preventing future disturbances, with "tales," idealized, imaginative constructions of the events as mythic encounters of villains and heroes. She thus posits an apt dichotomy between the historicizing and traditionalizing that followed major disturbances, although, surely, her characterization of chronicles as "realistic and precise" grants an unwarranted facticity to these necessarily partisan accountings (p. 172). The contributions to political culture of the popular "tales of protest" also seem ambivalent at best. They typically personified injustice as the wrongdoing of an alien villain and legitimated popular corrective action as that of righteous heroes of divine proportions. Could such a stylized displacement really have provoked an authentic political consciousness?

Indeed, one wonders to what extent the claim for an autonomous commoners' world, standing against an elite orthodoxy, is useful. Walthall's rich descriptions are most striking for the multiple motives and fluid social composition of protest in the decade. People's distance from elite forms and norms varied greatly, shifted frequently, and seldom cohered as a single, oppositional counterculture. Yet we can begin to debate the meanings and interests that motivated the extraordinary actions of the period only because Walthall has asked questions and tapped archival materials hitherto unexplored by Western scholars.

WILLIAM W. KELLY  
Yale University

SAJJA A. PRASAD. *The Patriotism Thesis and Argument in Tokugawa Japan*. Volume 3, *The Japanologists: A History*. Guntur, India: Samundraiah Prakashan; distributed by S. Prasad, School of History, Macquarie University, Australia. 1984. Pp. 215–633.

This book constitutes part 3 of Sajja A. Prasad's *The Patriotism Thesis and Argument in Tokugawa Japan*, originally advertised as "Studies in Sinological Sex, Religion, Racism and Nationalism." A critique of white Japanology will follow, but first we are likely to be treated to a forthcoming "not U.S.A. Rex, but U.S.A. Kleenex." What does one make of an author who flaunts his Harvard Ph.D. on the cover of his books, inflates his prose with a bombastic vocabulary sure to provoke outrage, and hates the Japanese nationalism he finds in his primary and secondary sources with a glowing passion that fires his rhetoric? And yet, Prasad is more well read about his subject than any other non-Japanese.

Indeed, Prasad, scholar-pundit and self-styled summarizer (p. 619), provides a voluminous summary of *kokugaku* (National Learning) discourse (and scholarship about it) as it reached full "patulousness" in the 1700s. He "perscrutates" the thought of Motoori Norinaga and his famous predecessors (hardly studied outside Japan): Keichū Ajari, Kada Azumamaro, and especially Kamo Mabuchi. Far from holding an "adiaphoristic" view anent the "intestinism" that constitutes *kokugaku's* characteristic "gibbosity" and that developed from Keichū to Norinaga and beyond to Hirata Atsutane, who "removed the morbid, infuscating encrustations on the original Japanese soul" (p. 254), Prasad directs plenty of "latration" at these thinkers and engages in endless "velitations" with scholars who celebrated them. He does this throughout the text and in no less than twenty-four appendixes, some of which are collations of notes to the main text, under headings such as "SS. Pound and Mabuchi."

This study must have been completed some time ago. The bibliography mentions nothing published after the late 1960s, and, although Maruyama Masao and Matsumoto Sannosuke figure among the targets of Prasad's critique, his wrath comes down mainly on prewar scholars who are hardly worthy of much attention.

With a generous amount of good will, one can mention three points concerning Prasad's argument. First, it is perhaps healthy that someone, through long quotations, vividly brings home the jingoism that was the driving force behind the otherwise noteworthy scholarly achievements of *kokugaku*. Second, although it has been made by others, the point that *kokugaku* was supportive, rather than subversive, of the Tokugawa government fully deserves to be restated. Third, Prasad's main thesis and perhaps most noteworthy contribu-

tion is his contention that *kokugaku* doctrine originated with seventeenth-century Shinto theorists. Historians, fascinated by the novel philological methods devised by *kokugaku* thinkers have often failed to pay due attention to the origins of the nationalistic doctrine that this philology validated in a new way but did not create.

Some specialists, fighting their bewilderment, may perhaps use this work as a short-cut to Japanese primary and secondary sources. Others will be totally lost because Japanese terminology is not always clarified.

HERMAN OOMS  
University of Illinois,  
Chicago

F. G. NOTEHLEF. *American Samurai, Captain L. L. Janes and Japan*. Princeton, N.J.: Princeton University Press. 1985. Pp. xiii. 386.

F. G. Notehelfer has written a well-meaning biography of a nineteenth-century "missionary" to Japan whose intentions remained an opaque mixture of ambition, self-aggrandizement, personal disappointment, and shrilly pious beliefs motored by unbending self-righteousness. The subject of this biography, Captain L. L. Janes, Notehelfer's "American samurai," is best known for his role as headmaster of the Kumamoto School for Western Learning between 1871 and 1876. But the real subject (in the philosophic sense) is the missionary, in Asia or elsewhere, as the agent of the modernist transformation of traditional societies. The importance of Janes's "mission" (he was not technically a missionary) is that he was one of the first foreigners to reside in the interior of Japan soon after the Meiji Restoration of 1868 to assume the directorship of a school pledged to preparing young samurai for the new society. Yet we are reminded that Janes was merely one of a score of missionaries who went to East Asia to preside as cultural midwives over the modernist births of these ancient societies.

Notehelfer contends that Janes's life before and after his eventful tenure in Kumamoto is equally fascinating for its unexpected turns and surprises, if not as important as his encounter with what came to be known as the Kumamoto Band of Japanese Christians. But this conviction, in coverage that is supposed to establish the unity of a life, ultimately works to convict Janes as something less than the accomplishments attributed to him by his biographer. Janes never sits comfortably for the portrait Notehelfer is trying to compose.

Be that as it may, Notehelfer has brought sympathy to his "American samurai" and respect for his achievements. He is convinced that, even though Janes was not a member of a church board, he

became "the most successful missionary in Japan." His apparent success stemmed from his years in Kumamoto where he is credited with having produced a remarkable group of Japanese trained in the Western idiom, who became leaders in the Japanese church.

After what can only be described as a privileged upbringing (Janes was a graduate of West Point) and a mediocre military career, and evidence, supplied by Notehelfer, that he suffered from psychological disabilities, Janes secured a post in Kumamoto to teach young samurai eager for a Western-style education. Although Kumamoto was located in southwestern Japan and at some distance from the metropolitan centers of Tokyo and Osaka, it was not a backwash. Before the restoration, the domain had been distinguished for a tradition of "progressive" thinking inaugurated under the tutelage of Yokoi Shōnan, one of the most remarkable intellectuals of a remarkable generation. Janes became the legatee of this tradition, inherited an outstanding group of young men already imbued with an enthusiasm for the new learning, and built on the received foundation. Precisely because of this combination of a progressive tradition in Kumamoto and the availability of a pool of gifted students, Janes was able to win for himself both a reputation that he could not have earned in the United States and the considerable talents of a later, admiring biographer. Janes's good fortune soured after he left Japan, and he soon became embroiled in a devastating scandal launched by his wife and her family, who charged him with adultery while in Japan. Notehelfer's fairness obliges him to provide a full (if not boring) account of this episode, which led to Janes's divorce, to convince us that although the accusations were probably untrue they nevertheless broke Janes and prevented him from acquiring an even more prestigious position in Japan. But this story is no Greek tragedy, and Janes is no flawed hero whose hubris brings down the wrath of the gods. Janes returned to Japan in the 1890s to deliver lectures at Doshisha University in Kyoto at the invitation of former students from Kumamoto who wished for the "opportunity to do justice to Captain Janes" (p. 230). But this moment dims as a doomed attempt to recuperate his earlier glory. Janes apparently contributed to the failure of his performance in Kyoto by deciding to mount a critique against Japanese imperialism and the vanished promise of social improvement he associated with the restoration.

When so much energy, industry, and care have been invested in rescuing such a small life, we are justified in recalling the larger story that serves to endow the biography with meaning. This larger plot that prefigures Janes's "life" concerns the story of missionary activity and the crucial role played by the

West in the transformation of archaic societies into the modern order. To trumpet the role of foreign missions in Asia in this manner by appealing to such "lives" as the transforming agents in the process is, of course, to diminish both the charges of imperialism associated with missionaries and the claims of native peoples such as the Japanese who clearly wanted to separate their activities from the "accomplishments" of foreigners. Notehelfer's life of Janes is supposed to reveal the truth of this narrative code that missionaries brought science, civilization, and even a new ethos (after all, what is the difference between an American samurai and a Japanese samurai?) to account for the successful modernization of Japan. Yet Janes's life is rarely able to fill the demanding role that the narrative has created for him and invariably fails to live up to the grandiose claims that attribute to his efforts "intellectual seeds that sprouted" and spread throughout the "world of business, finance, and politics" (p. 9). What seems closer to the truth of Janes's life is Notehelfer's assessment that people such as the captain were "handsomely paid, elevated to the status of semi-deities, heeded as if their voices resonated with the wisdom of Mt. Olympus" (p. 8). I would add, however, that they were less than ordinary men who represented only the flotsam and jetsam of Western societies, aimlessly drifting after having failed to find a place to land on Asian and African shores and to acquire an identity resembling Kipling's "The Man Who Would Become King."

H. D. HAROOTUNIAN  
University of Chicago

GREGORY C. KOZLOWSKI. *Muslim Endowments and Society in British India*. (Cambridge South Asian Studies, number 35.) New York: Cambridge University Press. 1985. Pp. x, 211. \$42.50.

This is a model monograph, using a well-chosen, specific topic to illuminate themes in a larger field: the modern history of Muslims in the Indian subcontinent. By focusing on a characteristic Muslim institution, *waqf* (plural *awqaf*), Gregory C. Kozlowski tells us a great deal not only about Muslim inheritance but also about everyday life, about the difference between Sharia and colonial law, about the new concept of a "Muslim community," and about politics after the end of the nineteenth century. Moreover, the book is brief, to the point, and clearly and engagingly written.

Kozlowski uses legal cases brought in connection with *awqaf* to illuminate both the intentions of the donors and the reasoning of the courts. The cases describe the personal connections, institutions, and cycle of rituals and events cherished by the donors;



they represent, Kozlowski suggests, "a very human attempt to oppose the forces of time" (p. 78).

Influenced by European presuppositions, British and anglicized, aristocratic Muslim judges assumed that the Sharia was a law with texts and a code of regulations. More appropriate, Kozlowski suggests, would have been an understanding of the historical role of Sharia in providing a range of past examples that served as a guide to conscience and a source of analogies. Experts in Sharia searched for models; those needing guidance could consult a variety of learned men, who very likely did not formally serve the state. The British, however, sought a fixed code, a definitive view, and, most distinctively, an unchanging, unevolving law. Given the variety of local customs widely practiced in relation to inheritance, the British enforced the Sharia more narrowly and more strictly than had Muslims themselves.

Europeans, classifying *awqaf* as public or private, secular or religious, attempted to make distinctions concerning endowments that grew out of their culture and had little meaning in the Indian context. Their main concern was to question the validity of *awqaf* whose beneficiaries were the donor's family. From 1879 on the High Courts overturned endowments considered to be of private, secular benefit. In this they were guided not only by distinctions important in their culture but also by the bias in British law of the time to free up all property for the market. They justified their position by Koranic injunctions that fixed inheritance shares for members of the family and limited amounts that could be willed as gifts. "For colonial judges, the simple prudence of the founders of *awqaf* began to take on the sinister cast of legal subterfuge" (p. 59).

In the judicial decisions and in the debates that followed, the British further assumed that religious affiliation defined social community, that there was a Muslim community united socially and adhering (or meant to adhere) to common norms. The *awqaf*, in contrast, show how significant various local and sectarian ties were in the conduct of everyday life. Muslims themselves, however, contributing to creating the reality the British assumed already there, learned to argue in public arenas on behalf of a united population. Saiyid Ahmad Khan, Ameer Ali, Shibli, and M. A. Jinnah differed with the British over the legitimacy of family *waqf* but claimed to speak for the "Muslim community." Evoking an argument they knew to carry weight, moreover, Muslim spokesmen argued that the practice of family *waqf* was "orthodox" and "ancient" and ignored the increasing use of *awqaf* in India to secure family interests in a period of substantial social change and insecurity.

Jinnah, as a newly elected member of the Governor-General's Council, was responsible for the passage of the Musalman Waqf Validating Act of 1913,

the first major victory of his political career. The act, above all because it was not retroactive, had relatively little effect. Kozlowski points out, however, that it provided a legislative victory for Muslim politicians, brought attention to Jinnah, and demonstrated the effectiveness both of arguments that claimed to speak for a single community as well as of cooperation between the modern and traditionally educated. The arguments and decisions concerning *waqf* reveal important themes in colonial culture. Kozlowski has expertly and articulately shown the significance of a little-known subject.

BARBARA D. METCALF  
University of California,  
Davis

KATHLEEN GOUGH. *Rural Society in Southeast India*. (Cambridge Studies in Social Anthropology, number 38.) New York: Cambridge University Press. 1981. Pp. xiv, 458. \$44.50.

Between 1951 and 1953 Kathleen Gough conducted anthropological field work in two villages within the Kaveri River delta of Tanjavur District, Tamil Nadu, India. Here, at long last, are her detailed descriptions of society and political economy in this major agrarian zone of south India during the period immediately after independence. The author has released this book as the preliminary offering in a two-volume project including results of follow-up field work conducted in the late 1970s.

Almost half of the present book is a detailed ethnography of Kumbapettai, an agricultural village that in the early 1950s preserved many of the features associated with the quintessential Tanjavur community: the slowly eroding ritual and economic dominance of Brahman families, the exploitation of large numbers of poor and ritually demeaned agricultural laborers, the steady progression of the seasons, and the concentration on rice cultivation amid irrigation channels. Gough has blended early land surveys and oral histories to trace the history of the village from its "foundation" in the late eighteenth century through the growth of bourgeois property and the movement toward a single-crop, market economy in the colonial period.

A less detailed survey of Kirippur looks at social and economic structures within a settlement near the coast, where modern irrigation improvements, labor migration, and changes in dominant caste groups created a more fluid and less stratified situation. In this area agricultural laborers under Communist leadership launched militant agitations in the 1940s and 1950s that led to legislation improving the lot of the poorest workers in Tamil Nadu villages. One of the author's main goals is to contrast



the initially quiescent political scene of Kumbapettai, where grinding class conflict appeared in the form of discrete struggles on a personal or caste level, with the environment of Kirippur, where greater social stresses emerging from the colonial impact created possibilities for mobilization of a laboring class.

Fully one-third of the book, including the first seven chapters and the conclusion, embeds the village studies within the historical experience of south India since the tenth century and within Marxian categories of class struggle. Basing her analysis primarily on older secondary sources and theories of the 1960s, the author unfortunately characterizes the crucial Chola period (the ninth to thirteenth centuries) as an example of the Asiatic mode of production. This presentation posits a bureaucratic, theocratic, hydraulic state and a "state class" that extracted large revenues from relatively undifferentiated peasant villages where private property did not exist; contemporary scholarship from a variety of directions has validated none of these interpretations. Gough views developments until the eighteenth century as variations on her Chola model, until the older "tributary" mode with its "state class" and "slaves" became energized by Europeans who introduced merchant capitalism. This model creates a particularly static view of the premodern past and tends, *à la* Wallerstein, to view south India within a world system without much emphasis on indigenous dynamism; it is an older approach that many contemporary historians are avoiding.

The primary subjects of this book are poor agricultural laborers—descendants of "slaves" who continue to support Tanjavur's agrarian system and who still suffer from extreme social disabilities. In this sense the author uses wider theoretical and historical questions, with statistical and ethnographic analyses, to frame the modern reality of the laborers. Gough's account of that reality, effectively spiced with true stories of village life and daily class conflicts, is a useful contribution to modern ethnography and to revolutionary struggle as well.

JAMES HEITZMAN  
*Library of Congress*

PORTIA ROBINSON. *The Hatch and Brood of Time: A Study of the First Generation of Native-Born White Australians, 1788–1828*. Volume 1. New York: Oxford University Press. 1985. Pp. xiii, 369. \$39.95.

Historians and other mythmakers are easily tempted to denigrate or romanticize the common people of former generations. Australians, confronted with the unpalatable complication that so many of their ancestors were convicts, have tended to portray the early inhabitants of Botany Bay either

as unregenerate riffraff or as refugees from economic oppression in Britain. These caricatures have been particularly unjust to the first generation of colonial-born Australians, and Portia Robinson has now sought to rescue this "long neglected race of men and women" (p. 5) from the condescension of their contemporary superiors and later historians. Through individual and collective biographies, based on rarely used sources and a mass of quantitative and qualitative evidence, she has brought into the historical spotlight, in all their fascinating diversity, the white children born in early New South Wales and also their parents, many of whom were or had been convicts. She stresses the stability of family life and the close relationships of children and parents. Contrary to the long-accepted stereotype, convict mothers were not depraved, reckless, and vicious but caring and concerned for their progeny. Infanticide was rare, rates of infant and child mortality were low, and exploitation of child labor unnecessary. Daughters remained at home rather than seeking outside employment; once married, they seldom worked for wages. They chose husbands likely to prosper irrespective of their convict past. Sons were keen to learn a trade or acquire land, and they eschewed seafaring. Robinson concludes that, far from displaying a hereditary taint of criminality, the native-born were "remarkably honest, sober, industrious and law-abiding" (p. 6).

This is a major work of revisionist scholarship, to be complemented by a separate volume of documentary evidence, but the study is not free from blemishes. The writing is at times repetitious, and many points are unduly belabored. There is a wearying stridency in the frequent claims to novelty of interpretation or use of evidence and in the reiterated determination to establish this forgotten generation's rightful place in Australian history. Robinson is excessively eager to put rival commentators in their place. Feminist writers are severely taken to task for depicting colonial women as victims of a male-dominated society and for thus exhibiting "excuse-explanation rather than historical questioning" (p. 66). The "class" attitudes of "respectable" contemporary observers, who saw New South Wales as a "moral dunghill," are roundly denounced when the duty of the historian should rather be to explain and seek to understand. At the same time, the memorials to governors, which of course stressed the respectability, industry, and worthiness of the supplicants, are treated too uncritically as evidence. Tasmanians, with their own convict inheritance, will be especially galled by the presumption that Australia is synonymous with New South Wales. The chapters, many with eccentric titles and all prefaced with commonplace quotations, often are excessively divided by subheadings, and the book concludes with an extraneous epilogue taken from *The Pil-*

*grim's Progress*. Four indexes are provided, but the influential moralist Samuel Marsden, castigated by Robinson for labeling convict women wanton and dissolute, manages to escape all four listings, although the clerical wit Sydney Smith is honored with a posthumous knighthood. A glossary of contemporary terms includes such esoteric entries as "government," "rising generation," and "climbing boys," and an irrelevant table converts imperial into metric measures. Cumulatively, the blemishes are irritating and detract from the pleasure of reading what is without doubt an impressive, authoritative study of a neglected generation.

PETER BURROUGHS  
Dalhousie University

D. W. A. BAKER. *Days of Wrath: A Life of John Dunmore Lang*. Carlton: Melbourne University Press; distributed by ISBS, Portland, Ore. 1985. Pp. xiv, 562. \$32.50.

Was it Sydney Smith who first asked why ruin a good review by reading the book? The 518 pages of closely packed text of D. W. A. Baker's study could intimidate the most ardent reviewer, the most dedicated student. The appearance of this scholarly work is formidable. It is, however, a "good" book, certainly not one with which to curl up on an empty evening but one in which the meticulous scholarship amply repays the time and perseverance necessary to reach page 518.

Australian colonial history as a respected and respectable discipline is of comparatively recent origin. The *AHR* itself has no Australian category for reviews of books. Baker clearly shows the maturity of Australian historians, the fascination and complexity of nineteenth-century Australian history, and the wealth and variety of sources.

John Dunmore Lang is a compelling figure who strode recklessly and fearlessly through life. Disputing, arguing, irascible, imprudent, inconstant and seemingly driven by an insatiable urge to speak, write, travel, and be at the heart of life, he was never a bystander, never a simple commentator, always a combatant. This man comes vividly, stridently, through Baker's biography. Not only Lang's characteristic traits and outspokenness but his Presbyterianism deeply embroiled him in political, religious, and social issues, making him a controversial and pivotal figure. He unhesitatingly served his God, his country, and his adopted land as he believed best, first as conservative, then as liberal, and finally as republican. His death in 1878 echoed the conflict, controversy, and emotions of his life. Did the public mourning at his funeral thinly disguise a great sigh of relief at his passing?

The form of the book is chronological. Thirty-four chapters follow the life of Lang through his childhood in western Scotland, his travels in England and America, and his intense involvement in the religious, political, and social life of Australia. It is, however, far more than a biography. Baker recreates with meticulous detail a fascinating epoch and introduces the persevering reader to the multifaceted societies of three continents. His vast canvas is peopled with obscure, notable, and notorious personalities and personages.

How can one explain the complexities and contradictions of Lang's responses and reactions to both his personal and professional life? Baker traces the influences of the man's ancestry and weaves what may have been inherited traits into the effects of travel, of life itself. In this analysis the reader may become aware of the dangers facing a biographer endeavoring to recreate the innermost thoughts, dreams, and motivation of the subject. Why, for example, was Lang "distressed" to observe that the women of Wick were employed gutting and packing fish (p. 278)? Such distress at traditional women's work would be more appropriate to a twentieth-century feminist.

This is minor criticism, as is the wish that a blue pencil had restrained some of the detailed descriptions that appear to add little to the book. For example, the depiction of the three kind women ministering to Lang's headache (p. 447) and the detailed account of the theft of the silver shoe buckles from his grandfather by a Highlander in the Pretender's army in 1745 (p. 279) are superfluous.

The book is well presented, as may be expected from the high standards maintained by Melbourne University Press. The annotations are a delight. The index is acceptable, although the absence of an entry "women" might have incurred the wrath of Lang. The lack of a formal bibliography is lamented.

This book may be recommended as a scholarly work to all concerned with the place and influence of man in his times. It is a work not to skim but to savor.

PORTIA ROBINSON  
Marquarie University

E. DANIEL POTTS and ANNETTE POTTS. *Yanks Down Under, 1941-45*. New York: Oxford University Press. 1985. Pp. xxiii, 455. \$29.95.

Since the outbreak of World War II, and increasingly in the 1970s, Australian and American policy makers have shown much awareness of each other, although Australian policy makers were thinking and talking about the United States far more than American policy makers were thinking and talking about Australia. In this sense Australia's relations

with the United States have been paradoxical. On the one hand, nothing could be more natural than a close relationship between two English-speaking peoples, both deeply involved with developments in the Pacific Basin, and with striking cultural, political, and geographical similarities; on the other hand, any relationship that evolved between them could perforce only be asymmetrical. This is not to say, however, that the relationship has not, from time to time, pulled in both directions; it clearly has. Nevertheless, as one senior Australian diplomat recently pointed out, "Great powers are not in the habit of taking too much notice of smaller powers." This is, one supposes, a fact of international life. Nonetheless, one hopes that members of the American academic community would take a somewhat different attitude toward one of Washington's most faithful allies. E. Daniel Potts's and Annette Potts's book is strongly commended to this audience.

In the dark aftermath of Pearl Harbor, the Australian prime minister John Curtin categorically stated, "I make it quite clear that Australia looks to America, free of any pangs as to our traditional links or kinship with the United Kingdom." In March 1942, after the arrival in Australia of General Douglas MacArthur from beleaguered Bataan, Curtin went perhaps a step further when he greeted the American forces as "visitors [who] speak like us, think like us, and fight like us and [from whom] therefore we can find a community of interest and comradeship."

In this book the authors tell the story of what it was like to be in Australia during World War II. Their main focus is the American presence, essentially the impact of two million American soldiers in a nation of just seven million. Until this study the story has been mostly told at diplomatic and military levels. Many myths and clichés pass for knowledge when discussing the American impact—better known to critics as the "American invasion." Other books in this field, such as John Hammond Moore's *Over-sexed, Over-paid and Over Here* (1981), are inadequate. To be sure there were problems, social and otherwise, as is always the case when the visiting army has more money and time on its hands. The authors have in great detail produced the definitive history of the social impact of these two million men and a few thousand women in uniform, their relations with the Australian community, and their lasting effects.

JOSEPH M. SIRACUSA  
*University of Queensland*

GLEN ST. J. BARCLAY. *Friends in High Places: Australian-American Diplomatic Relations since 1945*. New York: Oxford University Press. 1985. Pp. vi, 245. \$26.95.

Academic writing on Australian foreign policy has by now become fairly widespread, and Glen St. J. Barclay is a respected member of the community of scholars working in this field. His book's dust jacket pointedly asserts that this analysis is "pragmatic and non-ideological," and it is. Barclay quite properly has his opinions, but, unlike some writers in the field, he does not volunteer himself as apologist or debunker.

Barclay is a diplomatic historian, and his book reflects that perspective. It is strong on documentary research, including opened archives, private papers, and other primary sources. The writing is spirited, with frequent well-turned phrases and—perhaps to excess—a certain flamboyance in presenting the chief actors within the narrative. Much of the idiom is in fact one of a bilateral relationship taken as diplomacy. For Barclay, leadership counts for much, and recent history is prominently defined by its actors, including John Foster Dulles, Dean Acheson, and Dean Rusk on the American side and Herbert Vere Evatt, Percy Spender, Richard Casey, Robert Gordon Menzies, and Gough Whitlam on the Australian side. In following this tack, however, the book neglects domestic political cross-pressures and bureaucratic and institutional forces.

Barclay's subject has been dealt with before in various studies, but, apart from the focus on personalities and diplomacy that he favors, he brings to light some new material or reinterpretations that make his book welcome. This is not a study of Australian foreign policy at large. But, although the book is about the United States and Australia and concentrates more on diplomacy than on trade, law, or even detailed aspects of defense cooperation, it appropriately touches a broad spectrum of issues on which U.S. and Australian interests were clearly present or on which one side maneuvered to engage the other.

The breadth of the study prevents more than passing comment in this review on substance and argument. Several points—with which I agree—emerge as significant inferences and indeed could have benefited from more synthetic exposition. The first is that the conduct of Australian foreign policy, regardless of who is in charge, is intricately and ineluctably linked to American movements, both within and outside the context of the formal alliance. Second, although Australia is the weaker, more dependent partner in this asymmetrical relationship, special responsibilities of recognizing Australia's value and of sensitive handling of the relationship are incumbent on the United States. Finally, whatever one feels about particular features of Australian interplay with the United States, Australian policy has been neither mindless nor without careful rationale, even on the occasions when it has seemed to cleave to America. As has often been

argued, the elements of continuity in Australia's postwar foreign relations, including those with the United States, have reflected exceptional continuity. This has been by Australian choice, not through willful American instruction.

HENRY S. ALBINSKI  
Pennsylvania State University

RAEWYN DALZIEL. *Julius Vogel: Business Politician*. Auckland, New Zealand: Auckland University Press. 1986. Pp. 368. \$45.00.

Important as his career was for New Zealand's development in the 1870s and 1880s, Julius Vogel is a difficult subject for a biographer. His career in journalism and politics is amply documented, but his private life, personality, and motivations are often quite inaccessible. Yet such private material has come to seem essential in biographies. Biographies such as Robert Blake's *Disraeli* (1966) and W. K. Hancock's *Smuts* (1962) demonstrate not only the inherent interest but also the historical significance of such material.

Raewyn Dalziel has done much to overcome the shortcomings in her sources by providing vivid portrayals of the contexts in which Vogel moved: the London Jewish mercantile community of his youth, Victoria and Dunedin during the gold rush, the early capital at Auckland, even scenes of shipboard travel. More important, the extent and tone of colonial anti-Semitism are sensitively analyzed. The financial straits and bitterness of Vogel's last years are movingly told.

This book joins Keith Sinclair's works on William Pember Reeves and Walter Nash as the major biographies of New Zealand's historiography. Although it lacks the literary brilliance of Sinclair's books, Dalziel's is very readable, especially considering the great amount of detail she includes.

The heart of this biography is a detailed synthesis of the political history of New Zealand from about 1860 to 1890. A generation ago, in full cry against an earlier "whig interpretation," some New Zealand scholars of this period saw only shifting groups and factions and asserted that politics only concerned economic development. Dalziel does not discard these insights but works them into a more complex reality that again includes ideas and loyalties. The transition from provincial to national politics and the ambitious building of a national life through immigration and transportation—scandals included—are tellingly described through Dalziel's account of Vogel's career. A biography of Vogel is perhaps the best vehicle for this material—he was ever at the center of it all.

Dalziel's conclusion recounts the eclipse of Vogel's reputation after his death and its recovery starting

with Pember Reeves's historical works. Dalziel's critical but sympathetic account of Vogel completes the process. We can now appreciate the full significance of Vogel's career in New Zealand history.

JOHN A. WILLIAMS  
State University of New York,  
Stony Brook

## UNITED STATES

JOHN PATRICK DIGGINS. *The Lost Soul of American Politics: Virtue, Self-Interest, and the Foundations of Liberalism*. New York: Basic. 1984. Pp. xiii, 409. \$23.95.

John Patrick Diggins has written an ambitious, forceful, and complex book. Its purpose, he explains, is to explore a question central to the American republican tradition: whether Americans can "discover in the thoughts of the Founders a single first principle that would bind us to the past and restore the authority of older political ideas" (p. 4)? The question, he observes, is of vast importance and is much on Americans' minds in these years of bicentennial celebrations. The book constitutes his personal exploration of that urgent question.

Taking issue with many of the main themes of recent scholarship, Diggins argues that the United States has been preeminently "a liberal society, a society of free political institutions inhabited by individuals who love property and hate revolution." Since the nation's founding, American liberalism has embraced "radical means to achieve conservative ends" (p. 4). Both Jefferson's "liberal individualism," with its emphasis on "freedom, autonomy, and the sufficiency of the individual," and the "liberal pluralism" of Hamilton and Madison, stressing "power, stability, and the efficacy of the state," "identified happiness with property and material pleasure" and all but ignored "political ideals that appealed to man's higher nature" (p. 5).

Diggins does not so much celebrate liberalism's domination of American political culture as recognize its reality and examine its consequences for the nation's "moral vision." His interest is in the "moral content" of America's liberal tradition; his task is to examine how the articulators of that tradition in the early republic dealt with "the ethical contradictions of capitalist culture." Such issues, he believes, "have generally gone unexplored in traditional historiography," whether developed by students of classical republicanism, Scottish moral sense philosophy, or Marxism.

The nation's founders, Diggins explains, joined Lockean liberalism with a Christian sense of human fallibility, thus creating an "interrelated tradition"



that emphasized the values of liberty and property and the satisfaction of earthly desires but contained as well a profound sense of human imperfection, a religious sensitivity to meaning and authority beyond the conditions of daily life. The search for a spiritual balance to the consuming materialism of an increasingly commercial society preoccupied nineteenth-century theorists and was most eloquently expressed in the writings of Melville and Lincoln. Those two, Diggins laments, were perhaps the last to grasp the fundamental tensions of American political culture, to search beyond the shallow, materialistic surface of liberal life for a vision of the nation's moral meaning. They were the last to seek out what has since become "the lost soul of American politics."

What is one to say about such a sprawling and intensely personal book? Certainly, it constitutes a scholarly tour de force, for it engages an impressive array of writers from the late eighteenth and nineteenth centuries. But in that strength is to be found the book's major difficulty as well. Roaming so widely, encompassing so many texts, Diggins often takes too little time to explicate those texts, to establish the evidential basis for his interpretations of them. His commentaries are confident and vastly informed; one must take seriously what he has to say about Jefferson and Hume, Machiavelli and John Adams, Thoreau and Locke, Tocqueville and Henry Adams, Melville and Lincoln. The distance between commentary and documentary base, however, often remains frustratingly wide, leaving the skeptical reader with little option but to pull Diggins's texts off the shelves and read them with his arguments in mind.

Ultimately, the book is as much polemic (in the nonpejorative sense of the word), as much Diggins's debate with recent historiography and personal commentary on the tortured path of American public morality, as it is written history. This testimonial quality generates the book's passion and explains much of its fascination.

JOHN R. HOWE  
*University of Minnesota*

BERNARD SCHWARTZ. *Some Makers of American Law: Tagore Law Lectures*. Calcutta: Ajoy Law House or Oceana, Dobbs Ferry, New York. 1985. Pp. 14, 182. \$30.00.

Bernard Schwartz, a leading authority on American constitutional law, delivered the chapters of this book as the Tagore Law Lectures at the University of Calcutta. The "lawmakers" discussed are: George Mason, John Adams, James Madison, John Marshall, James Kent, Joseph Story, Lemuel Shaw, Thomas M. Cooley, Oliver Wendell Holmes, Jr.,

Roscoe Pound, Benjamin N. Cardozo, Arthur T. Vanderbilt, Roger J. Traynor, and Earl Warren. They are grouped according to four stages in the development of the American legal tradition. Mason, Adams, and Madison belong to the nation's founding period prior to 1789. Marshall, Kent, Story, and Shaw are considered shapers of the formative era of American law, 1789–1865. Cooley, Holmes, Pound, and Cardozo, classified as the critics of the Gilded Age, 1865–1929, are presented as the founders of modern law, 1929–70, whereas Vanderbilt, Traynor, and Warren are the most significant practitioners of the modern era. Schwartz also suggests that, at least since the 1970s, the nation's law, like the rest of American civilization, has been going through a postmodern search for a new synthesis.

In keeping with the occasion on which they were given, the lectures are laudatory and uncritical. Because nearly every one of these individuals made significant contributions to their age, Schwartz's approach is understandable. Such a treatment is, however, one dimensional. Holmes, for example, was not only a truly original legal thinker, as Schwartz says, but also a profound skeptic who looked on the majority of American voters as "thick fingered clowns." In accordance with this distrust of democracy, followers of Holmes such as Felix Frankfurter used his ideas to argue for government by bureaucrats. The analysis of each of Schwartz's figures suffers from the same one-sided examination; as a result, the individual's place in time, like the times themselves, are only partially examined.

But the style is highly readable, insightful, and enlivened by vivid anecdotes.

TONY FREYER  
*University of Alabama*

JAN C. DAWSON. *The Unusable Past: America's Puritan Tradition, 1830–1930*. (Scholars Press Studies in the Humanities, number 4.) Chico, Calif.: Scholars. 1984. Pp. viii, 155.

A chronicle of transformation and decline, Jan C. Dawson's study traces the vicissitudes of the Puritan tradition through the first post-Puritan century in America. Perhaps Dawson's description of the "core" of Puritanism—an activist "longing" for "creation redeemed and perfected" (p. 6)—is rather general. But the various invocations of the example of the Puritans over time are, as a rule, well specified.

The first half of the book treats the ascendancy of an ideology that looked to the Puritans for the legitimation of causes justified by "higher law" such as abolition (p. 44). Romantic historians such as George Bancroft contributed to the propagation of



this ideology: "The issue of Puritanism is popular sovereignty" (p. 27). Dawson also sets forth the attacks on this ideology from Roman Catholics and Copperheads as "Puritanism in Politics" (p. 57) and from southerners as "the triumph of the profane over the sacred" (p. 61). (That these polemics were not disinterested is a bit muted.)

The second half of the study begins with the post-Civil War failure of the genteel tradition to draw adequate sustenance from the Puritan legacy then shows how a new generation of historians (especially Brooks Adams and C. F. Adams, Jr.) discovered elitism and persecution in colonial New England. By 1906 Theodore Clark Smith could observe that historians had difficulty writing about "the Puritan mind . . . without caricature or repugnance" (p. 91).

Those qualities marked some of the negative judgments of early twentieth-century cultural commentators such as Van Wyck Brooks, Randolph Bourne, and especially H. L. Mencken, who were seeking other versions of a "usable past" (p. 119). As Dawson notes, Puritanism was still a vital enough presence to draw fire. But, not long after Stuart Sherman tried to establish the Puritan tradition as a bulwark against Modernism in the 1920s, the tradition was exhausted as a cultural force (though just about to gain renewed interest, of course, as a subject of scholarship).

Dawson's compact argument is clear in outline, and it contains ample and sometimes ironically telling detail, such as praise in 1892 for Henry Ward Beecher as "a Puritan of the Puritans" (p. 87) or wishful attempts in the 1920s to make a Puritan out of Walt Whitman. A tendency to relate all aspects of American culture to Puritanism exaggerates its significance, but Dawson's introduction offers the right perspective: "This study is based on what the relatively few Americans who participated in the development of the Puritan tradition said about Puritanism and why" (p. 7). That qualification kept in mind, readers can learn a good deal from this book.

HOWARD KERR  
University of Illinois,  
Chicago

ROBERT M. TAYLOR, JR. and RALPH J. CRANDALL, editors. *Generations and Change: Genealogical Perspectives in Social History*. Macon, Ga.: Mercer University Press, 1986. Pp. xv, 332. \$28.50.

In the nineteenth century genealogists and historians in the United States worked closely together. As the study of history became more academic and professionalized, the interaction between these two sets of researchers diminished until they had little contact. During the past twenty-five years, however,

genealogists and historians have again begun to collaborate because of the increased use of family reconstitution techniques in the analysis of American social and demographic history. Unfortunately, few attempts have been made to encourage the exchange of information and techniques between them despite the expansion of informal contacts.

This volume of essays is one of the first attempts to bring together the work of genealogists and social historians in the United States. Although most of the essays have been written by professional historians, the book should be useful to both groups because it demonstrates the broader context in which family genealogies can be used. Of the sixteen essays, twelve have been written for this collection and some of the reprinted essays first appeared in publications that most historians do not follow.

The editors, Robert M. Taylor, Jr. and Ralph J. Crandall, provide a historical account of the interactions of historians and sociologists that emphasizes their similarity of interests. This is followed by an essay by Samuel Hays that details the use of genealogies by social historians today. John Adams and Alice Kasakoff contribute a chapter that discusses the reliability and representativeness of genealogies for the investigation of family life in the past, and Robert Anderson calls for the introduction of genealogical courses into the curriculum of departments of history or of sociology. James O'Toole describes how historians and genealogists can reconstruct Catholic family histories, and Elizabeth Mills cautions us to approach southern family life from a broader ethnic perspective. Andrejs Plakans addresses the use of genealogies in Germany to study kinship ties and the survival of lineages in a particular village.

Several of the essays illustrate the usefulness of genealogies for reinterpreting social history. John Schutz discusses the contributions of genealogy to the study of colonial legislators in Massachusetts, and Dean May and his colleagues have produced a fine study of the surprising relative stability of Mormon communities in the nineteenth century. Patricia O'Malley explores marriage patterns in early Rowley, Massachusetts, and Lawrence Kilbourne analyzes the decline in fertility in Hampton, New Hampshire, during the second half of the eighteenth century. There are two interesting studies of child-naming patterns—one by David Fischer on Concord, Massachusetts, over two centuries and the other by Darrett Rutman and Anita Rutman on colonial Middlesex County, Virginia, from 1650 to 1750.

The volume concludes with three essays on migration based on genealogical information. Virginia Anderson studies the migrations of three generations of the Danforth family in New England and urges us to look beyond the confines of individual

community studies. Claudia Bushman investigates the movement of the Wilson family from Delaware to Philadelphia and finally to Indiana. Her essay illustrates how the strain of spatial distance coincided with the widening social distance between these migrants and their relatives in the East as the Wilson family failed to prosper in their new home in the Midwest. Finally, Walter Kamphoefuer discusses the problems and opportunities of tracing Germans immigrating to Missouri in the nineteenth century.

Overall, this is a very useful and thoughtful collection of essays on genealogies as a source of data for the analysis of American social and demographic history. Although the essays vary considerably in their use of sophisticated techniques of statistical analysis as well as in their substantive contributions, both historians and genealogists should find them quite helpful.

MARIS A. VINOVSIS  
*University of Michigan*

MICHAEL B. KATZ. *In the Shadow of the Poorhouse: A Social History of Welfare in America*. New York: Basic. 1986. Pp. xiv, 338. \$22.95.

Simply put, the thesis of this ambitious and very successful work is that, for all its incoherence and irrationality, the American social welfare system has resisted fundamental change. It has done so, and continues to do so, because it was designed to serve a consistent and useful, however contradictory, set of purposes: the relief of misery, the preservation of the social order, the regulation of the labor market, and the mobilization of political power. The strength and persistence of the welfare system have been derived "from its symbiosis with American social structure and political economy," to use the author's words (p. ix).

This, of course, is not a novel proposition. Indeed, students of American social welfare history and policy will find much in this work that is familiar. Michael B. Katz borrows heavily from other scholars of the subject, including Raymond Mohl, Paul Boyer, David Rothman, Roy Lubove, William Graebner, James Patterson, Frances Piven, and Richard Cloward. He also relies a good deal on his own previously published work, especially on the demography of poorhouse populations and other related matters.

This is not to say that there is nothing new in this "social history of welfare in America." Thus, for example, Katz's critical comments about the widely applauded New York State Care Act of 1890, which for the first time provided complete state care for the insane poor, are revealing. So, too, is his argument that the removal of children from poorhouses

in the latter nineteenth century, another broadly acclaimed development, was designed more to break up poor families than to help needy youngsters. His discussion of the negative impact that the lack of a permanent governmental infrastructure had on the American welfare system during the Great Depression also is an important contribution, as is his informative analysis of a closely related matter—the shortcomings of public works as a welfare policy. Katz is at his best, however, in the last part of the book, entitled "From the War on Poverty to the War on Welfare," a brilliant account of the more recent assault by the localities, the states, and especially the federal government on what he refers to as the "semi-welfare" state that had emerged in America by the 1960s.

Katz's major contribution is to synthesize an enormous amount of material, some of it new and much of it not, in a very readable, meaningful, and persuasive manner. Indeed, so persuasive is he that the reader is left in a rather uncomfortable, if not enraged, position. As Katz makes clear, welfare policies and practices in America have not been inevitable; they have been the products of choices made among possible alternatives. As he makes equally clear, America has the resources and the knowledge to eliminate poverty and dependency. Yet readers of this book can only conclude that the nation does not, and will not, have the will to do so. Most Americans will continue to deny that poverty and dependency are structural in nature, products of the political economy. And, at best, the nation will maintain a partial welfare state that continues to make clear distinctions between social insurance and public assistance and provides more aid and security to the middle and upper classes than to those who need them most.

WALTER I. TRATTNER  
*University of Wisconsin,  
Milwaukee*

RONALD P. FORMISANO and CONSTANCE K. BURNS, editors. *Boston 1700–1980: The Evolution of Urban Politics*. (Contributions in American History, number 106.) Westport, Conn.: Greenwood. 1984. Pp. viii, 296. \$35.00.

With the loss of a synoptic vision of urban history in the 1950s, historians abandoned the effort to produce single-author "urban biographies" covering the entire span and scope of a city's history. Editors, teachers, the promoters of civic anniversaries, and—it is presumed—general readers continue, however, to ask for comprehensive works on individual cities. In response, several groups of historians have revived a nineteenth-century form, the municipal *Festschrift*. Among recent examples of this genre, the

present book is unusually coherent. Inevitably, it lacks the unified vision and strongly argued thesis that a single author might have supplied. But it does offer a single focus: the changing institutional and social bases of municipal politics. Its valuable and sustained contributions to this subject largely compensate for its resolute omission of Boston's economic, social, or cultural history.

The most important essays examine Boston municipal politics between 1800 and 1910. Even G. B. Warden, who deftly, if too briefly, sketches the history of the city's eighteenth-century politics, opens his account with a reference to the Irish potato famine of 1847. Ronald P. Formisano, who picks up the story in 1800, offers a preliminary discussion of the shift, in the 1830s, from "deferential-participant" to "party" politics; this shift was accompanied, he suggests, by a partial reduction in the power of the economic and social elite. Frederic C. Jaher shows that the brahmin elite, however defined, continued to participate actively in politics and to dominate professional, educational, and charitable organizations up to 1860. In complementary essays three historians extend the point, arguing that brahmins played a pivotal role in Boston Democratic politics until 1910. Geoffrey Blodgett provides a concise statement of his argument that "gentle reformers" of Yankee origin gave the Irish immigrants effective leadership until the late 1890s. Paul Kleppner offers the only well developed statistical argument of the book in support of his observation—perhaps the most original in the collection—that before 1910 factionalism made any given Irish candidate for mayor less attractive to Irish voters than a diplomatic Yankee. And Constance K. Burns insists that the city's Progressive movement briefly tried to transcend class and ethnic limits, before failing decisively in the mayoral election of 1910.

The four remaining contributors muse on broader themes in Boston's twentieth-century politics. Charles H. Trout provides a stimulating and readable corrective to the account of the charismatic James Michael Curley contained in *The Last Hurrah*. Martha Weinberg seeks to explain the more recent successes of Kevin White. Journalist J. Anthony Lucas finds it notable that the key protagonists on both sides of the busing controversy of the 1970s were of Irish descent. The book would have been stronger if Lucas (and Burns and William V. Shannon) had considered Blodgett's and Kleppner's more complex interpretation of Yankee-Irish relations, and if all the authors had shown that mayoral battles actually made any difference to policy, or to ordinary citizens. To vindicate its ethnic and cultural emphases, the book should have paid far more attention to possible economic factors in politics.

Only a *Festschrift* of unusual quality and consistency, however, would invite criticisms of this sort.

DAVID C. HAMMACK  
Case Western Reserve University

JAMES C. KLOTTER. *The Breckinridges of Kentucky, 1760–1981*. Lexington: University Press of Kentucky. 1986. Pp. xviii, 393. \$35.00.

For almost two centuries each generation of the Breckinridge clan of Kentucky and surrounding states has produced individuals of varying degrees of local, regional, and national prominence. Moreover, the history of the clan is copiously documented. The Breckinridge papers at the Library of Congress alone, according to James C. Klotter, consist of 515 bound volumes plus 350 boxes of unbound material, and substantial collections of papers exist elsewhere. These facts, plus the current interest in social history and recent breakthroughs in family history, make the Breckinridges an ideal subject for social and family historians.

Klotter has, however, largely foregone the approaches and concerns of the "New Social History" (p. x) and relies instead on those of "old style" family history" (p. ix). The result is a collection of conventional biographical essays on the eight most prominent Kentucky Breckinridges, beginning with John (1760–1806), who founded the Kentucky branch of the family, and ending with John B. (1913–79), a three-term congressman. The essays focus not on family matters as such but on the "development and evolution of (1) political philosophy, (2) racial and social attitudes, and (3) intrafamily relationships" (p. x). This necessarily turns attention to the public lives of the eight individuals and to their relationships with immediate relatives and away from things of concern to family historians today—life course, family dynamics, child rearing, intergenerational relationships, evolution of the family, and, above all, the patterns in those things and how those patterns change over time. Only in the essay on Sophonisba Breckinridge, and then only briefly, does Klotter address any of these topics systematically as opposed to anecdotally.

Klotter's work is therefore family history chiefly in that it narrates the public lives of several people named Breckinridge and remarks on their relationships with some of their relatives. As such the work has usefulness, but the limitations of the approach outweigh Klotter's achievement. The reader is, for example, taken through the Civil War in the essay on Unionist Robert J. Breckinridge, then again in that on Confederate John C. Breckinridge, and still again in that on Confederate W. C. P. Breckinridge, Robert J.'s son. Not only is this bewildering chronologically but it precludes systematic discussion of

the most obvious and relevant question: how did this extended family (as opposed to a few of its members) accommodate to the stresses and disruptions of what for its members was literally a brothers' war?

Similarly, none of the Breckinridges was of first-rank significance, and the most important of them, Vice-President John C. Breckinridge, is already the subject of conventional biographies, which Klotter does not supersede. The significance of the family for historians is not in its individual members but in the fact that so many Breckinridges achieved so much over so long a period of time. A study of the extended clan itself rather than of individual Breckinridges is surely the best way of explaining this significance. Klotter has brought together a good bit of information on individual Breckinridges, but the history of the family remains to be written.

A. NEWBY  
University of Hawaii

PHILIP L. BARBOUR, editor. *The Complete Works of Captain John Smith (1580–1631)*. In three volumes. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1986. Pp. lxii, 448; xii, 488; xi, 513. \$150.00 the set.

After writing the best modern biography of John Smith in the 1960s, Philip L. Barbour became convinced that the feisty Elizabethan adventurer deserved a modern edition of his works. The last had been compiled in the 1880s by Edward Arber, and it was both incomplete and dated in its editorial apparatus. Arber omitted Smith's letter of 1618 to Francis Bacon (although he printed it in another form), the *Sea Grammar*, a number of passages by Smith from Samuel Purchas's *Hakluytus Posthumus*, and some recently discovered poetry by Smith. Barbour's new edition includes these materials. He is of the middling school of annotation: enough information to set the material context and clarify obscure words and passages but not so much as to obtrude on the integrity of Smith's work. The result is an attractive three-volume collection, intelligently annotated, handsomely printed and bound, and meticulously copyedited. It is, in short, a creditable addition to the letterpress editions of the works of important historical characters.

And yet, one might ask whether the achievement has been worth the expenditure of resources. Will this work change appreciably the scholarship on Smith or on the early years of colonization? It seems unlikely. This new edition, though more complete and admirably edited, contains no startling revelations. The older work, for all its defects, remains serviceable. On the basis of it, such authors as

Bradford Smith, Barbour himself, Everett H. Emerson, Alden T. Vaughan, and others had, in recent years, built up a body of literature much advancing our knowledge of Smith and his times. This new edition will probably not make a decisive change in Smith scholarship. But why cavil? Completeness must be preferred to incompleteness and accuracy to even occasional inaccuracy.

One other point should be made. This edition occupies three substantial volumes, testifying largely to Smith's devotion in his later years to publishing. He was not, however, a prolific writer. His own original writing constitutes a relatively small corpus. He reprinted and rewrote materials many times over. Some of the changes that occur in his work over time are significant, but many more are not. It is difficult to see what strategy the editors might have adopted save printing the whole of his work. But the result of having done so is a quite repetitious collection, interesting mainly to the biographer concerned with the provenance of Smith's various versions of his writings.

But, for all this, Smith remains a character of unsurpassed interest, and his works a mine of material for the early history of exploration and colonization. As Barbour aptly notes, he was a man of many talents: autobiographer, compiler (as distinct from editor), geographer, ethnographer, soldier, governor, sailor, admiral, and trader. Although his own inclinations were toward the world of action, he spent the later years of his life in involuntary retirement, writing and compiling accounts of his own and others' past exploits and futilely seeking employment in the business of colonization. In truth his active career in the New World was relatively short. He stayed nearly two-and-one-half years in Virginia, for approximately one of those years in command of the colony. Thereafter he managed a voyage to New England in 1614; two attempts the following year were aborted and, similarly, one in 1617. So ended his career of active adventure. He turned thence to his books. Thus, Smith was in many ways a disappointed man, whose life, not uncharacteristic of the Elizabethan and Jacobean age (witness Sidney, Essex, and Raleigh, to name just three), started in a blaze but rapidly dwindled in intensity.

About America Smith held seminal views. Although he wrote his share of promotional literature, he avoided promises of quick wealth and stressed instead the long-term potential of the continent under the exploitation of talented and industrious settlers. He was particularly taken with the prosaic but quite real wealth that awaited fishermen in the northern seas. In a moment of frustration, it is true, he might have lamented the failure of the English to stumble on the fabulous treasure that greeted the Spaniards in Mexico and Peru, but his own vision,

though more ordinary, was more realistic. He anticipated for the new land an early version of the American dream. America provided an opportunity to renew English society from below. Independent people, masters of their own labor and land, rewarded for talent and industry instead of position, would escape the social limitations of Europe and grow rich in the New World. It did not work for Smith, but it would for others.

Barbour reiterates his belief in Smith's veracity. He may have been prone to exaggeration and to the promiscuous and unattributed use of other people's material, but on the major events of his life he can be depended on. He did indeed fight in the Turkish wars, and he was saved by Pocahontas despite his failure to mention this incident until much after its occurrence and after Pocahontas's death. Perhaps so, although on the Pocahontas story the revisionist argument does ring a bit hollow. We have long been told that Smith omitted Pocahontas's role in the episode from *A True Relation* because he wished to stay in the company's good graces by avoiding reference to Indian hostility. But the work contains other and more telling evidence of native hostility, and Smith did not intend any of it for publication, which leaves one wondering whether in his later, more formal, *General Historie* he did not succumb to the temptation to embroider a good story. Fortunately, the value of Smith as a source does not hinge on his relationship with Pocahontas.

The native people loom large in Smith's writings. He saw them as savages, an "inconstant" people whose manner of life contrasted starkly with the civil order of England. He was not beyond associating the savage condition with bestiality. But for Smith the categories did not always serve. He found occasional virtues in native life and thought these evidences of civility. But never did he doubt the hostility or potential hostility of the Indians. He believed that his military experience in Europe equipped him to keep them in subjection. As the colonies developed, he saw the value of employing native labor. Although he wished to see the Indians converted to Christianity, he was not a major advocate of proselytization. Religion generally rested lightly on Smith's writing, although he adopted Bradford's contention that the plague that afflicted the natives of the New England coast had been sent by God to make way for the English. Perhaps the most significant aspect of Smith's dealings with the Indians, and certainly the most important for modern scholars, was his interest in ethnology. He took care to describe the native way of life, their numbers, artifacts, and languages. And he appears to have gained a usable knowledge of the Algonquian spoken by the Powhatans. On Smith's treatment of

the Indian, Barbour's editing is especially informative.

BERNARD W. SHEEHAN  
Indiana University

OLIVER A. RINK. *Holland on the Hudson: An Economic and Social History of Dutch New York*. Ithaca, N.Y.: Cornell University Press, for the New York State Historical Association, Cooperstown, N.Y. 1986. Pp. 285. \$29.95.

In this book Oliver A. Rink explains the history of Dutch New York not only from the traditional perspective of the West India Company's administration but also from that of the role of a few private merchants of the metropolitan country. The growth and development of New Netherland during the last years of Dutch rule largely resulted from private initiative after the company was forced to accept a diminished role. Up to 1639 the bizarre administrative apparatus of the company prevented reasoned analysis of problems so that prompt, decisive action from the homeland was seldom forthcoming during crises in the colony.

In the first half of the book Rink discusses the institutional framework of the company and describes rivalries in the Netherlands as they related to trade and colonization. He then capitalizes on a new source, notarial records in Amsterdam, a treasure of information on the commercial ventures of traders in Holland who came to dominate traffic with New Netherland. These sources also shed some light on later migration to the colony. Analysis of samples of two groups of immigrants, one for settlers before 1644 in Rensselaerswyck, a seignorial patroonship, the other from about one thousand passengers arriving in twenty-five ships between 1657 and 1664, shows that in the earlier migration young, single men dominated (60 percent) with families in the minority. Among these settlers were foreigners—Norwegians, Swedes, Danes, French, and some Africans. Without the gold or glory to be won elsewhere in the Western Hemisphere, New Netherland fared poorly in the competition for settlers with the other portions of the Dutch empire. Those who came to the colony on the Hudson often were men with few ties to the United Provinces, foreigners drifting in and out of Dutch territories, members of ethnic and religious minorities, unemployed sailors, soldiers, adventurers, and dispossessed farmers. By contrast, among the immigrants arriving between 1657 and 1664 only a quarter were single men whereas nearly 70 percent belonged to families, for the most part young couples with children. As a result of this new immigration, the population stood roughly at nine thousand souls by 1664.



An analysis of voyages to New Netherland (based on 129 bottomry bonds drawn up by notaries in Amsterdam) affords insight into the economy of the colony. Once the States-General forced the West India Company to relinquish its hold on trade with New Netherland in 1639, the last twenty years of the colony's existence as a Dutch possession witnessed not only a growing population but also a rising trade in provisions, fur, hides, tobacco, timber, and slaves. This trade was dominated by a small number of merchants in Amsterdam, who allowed no room for indigenous colonial traders. The merchants of Amsterdam controlled the transoceanic traffic, set prices for European goods sold in the colony, recruited settlers for New Netherland, and exploited the colonists. Once the English took over New Amsterdam, their acts regulating trade and shipping eliminated Amsterdam competitors from the carrying trade. This point is significant particularly because some scholars have suggested that had it not been for the English Acts of Navigation, which channeled enumerated products through the English entrepot, planters on the Chesapeake could have used Dutch rather than English middlemen. But if the merchants of Amsterdam were so exploitative in their relations with their compatriots in New Netherland, as Rink argues, would they have been any more generous with English colonists? Given that the Dutch settlers were farmers, artisans, and laborers—men without capital, credit, or knowledge of and contacts with distant markets—would a local trading community have developed during these years?

Rink concludes that the growth of population and trade suggests New Netherland was a successful experiment in private colonialism under the auspices of a chartered monopoly, for triumphs as well as tragedy filled the last years of New Netherland. As the number of immigrants rose, the chronic problem of underpopulation seemed solved, the colonists attained institutions for local government, and the firm, if autocratic, hand of Pieter Stuyvesant gradually brought political stability. Yet from another perspective was not New Netherland a failure? When an English expedition of three hundred soldiers in four ships appeared in 1664 the director-general received no support from the local populace. Stuyvesant attempted to whip up the settlers to fight for their homes, their colony, and their country, in vain. A delegation of ninety-three citizens demanded that he immediately surrender. With none to stand with him, Stuyvesant yielded. New Netherland was more successful than previously thought as a colonizing and trading venture, as Rink argues, but perhaps not as an endeavor in Dutch empire building.

J. M. SOSIN  
University of Nebraska

DAVID W. GALENSON. *Traders, Planters, and Slaves: Market Behavior in Early English America*. New York: Cambridge University Press. 1986. Pp. xiv, 230. \$34.50.

David W. Galenson is an accomplished economic historian who specializes in the British colonies during the seventeenth and eighteenth centuries. His publications to date have concentrated on the demand for and supply of labor, white and black, in the emerging plantation economies. Unlike too many economists, Galenson has worked consistently and carefully among the writings and the records of the time and place and has developed a sensitivity for his materials, his people, their era, and their lives. His research and his arguments thus deserve the respectful attention of all who share his interests even though some may argue with his conclusions. He is not to be dismissed lightly, if at all.

Galenson's new book rests on an analysis of the prices of slaves sold at Barbados by the Royal African Company (RAC) between 1673 and 1723. His data come from the account books of the RAC in the Public Record Office, London; they record the price at which the RAC's agents sold roughly three-quarters of the slaves that company ships discharged at the island. The RAC delivered less than half the slaves landed at Barbados over the period; even during the 1680s, its best decade, the RAC accounted for only 80 percent of the total. The book describes in rich detail the workings of the slave trade to the New World, the role of the RAC in that trade, and some of the consequences of the trade for those involved in it. It is thus a complement to, but in no way a replacement for, K. G. Davies's magisterial *The Royal African Company* (1957) to which Galenson handsomely acknowledges his (and our) considerable debt.

Galenson's study is much more than simple description. From his analysis of the data on sale prices and the inferred behavior of the sugar planters and slave traders, he derives three conclusions: first, that both planters and traders were "diligent and systematic" in their pursuit of "profit maximization" (p. 143); second, that, despite Adam Smith's portrayal of the collapse of the RAC as the classic example of the devastating economic results of government-chartered monopolies, the failure of the company was a consequence of competition in the slave trade that the company was unable to deal with adequately; and third, that large, competitive markets functioned in the seventeenth century. This last conclusion, which contradicts Karl Marx's contention that the role of markets was minimal in English society until the Industrial Revolution, Galenson considers his most striking result: "although some economists and historians have suspected that large competitive markets existed at this time, it has not

previously been realized how complicated and sophisticated their behavior was" (p. 153). This very important book thus interjects itself into the heart of the current debate over the mind-set and motives of colonial producers and consumers. It is an impressive contribution.

JOHN J. MCCUSKER  
University of Maryland

DOUGLAS EDWARD LEACH. *Roots of Conflict: British Armed Forces and Colonial Americans, 1677–1763*. Chapel Hill: University of North Carolina Press, 1986. Pp. xi, 232.

The title and subtitle of Douglas Edward Leach's short book accurately summarize its argument: that the repeated interaction of British and colonial American armed forces was a major cause of the breakdown of the empire in 1775–76. From the belated arrival in Virginia of one thousand British soldiers to put down Bacon's Rebellion in 1677 to the complaint of General Jeffery Amherst in 1762 about "the Infamous Trade" (p. 162) of American merchants with the French enemy, military operations embittered relations between the metropolitan country and its colonies. The failure of small British garrisons to maintain the Andros regime in 1689, the tragic Walker expedition against Quebec in 1711, Oglethorpe's failure at St. Augustine in 1742, the nightmarish Caribbean campaign of 1740–42, the grim realities behind the spectacular capture of Louisbourg in 1745—these are the chapters of a story with a single theme: the repeated estrangement of Britons and Americans engaged in military activities. The climax comes, of course, between 1755 and 1763, when for the first time large British forces took over much of the fighting in America and in a series of campaigns ended the French and Spanish threat. Although the last, greatest colonial war was a decisive Anglo-American victory, it also entailed unprecedented levels of friction between the victors. The resultant bitterness underlay both British and American attitudes and behavior in the postwar period and contributed to the outbreak of the revolution.

This is a familiar story, but never before has it been told so authoritatively. Leach has exploited published and manuscript sources from both sides of the conflict and provides in his text and notes a wealth of anecdotal detail. If at times his generous use of adverbs and adjectives imputes motives or consequences to events for which the evidence is silent or vague, the story itself is based on solid fact. Again and again, military plans went awry, British officers fumed, Americans refused to obey, and even victory could not dispel the feelings of mutual disgust.

The story, however, lacks the balance necessary for a satisfactory historical explanation. All imperial relations are fraught with metropolitan arrogance and colonial resentment. All warfare—but perhaps especially colonial war in the eighteenth century—is a story of failures, often of disasters; before steam power and electrical communications, military operations overseas were almost impossibly difficult. Was the Anglo-American military experience exceptional, or does the American revolution only make it seem so? A Virginia mansion named after the villainous Admiral Vernon and a New England college named after the arrogant General Amherst suggest that the Anglo-American relationship was actually less one-dimensional and more complex than is allowed by the story told in this valuable book.

JOHN SHY  
University of Michigan

GRAHAM RUSSELL HODGES. *New York City Cartmen, 1667–1850*. (American Social Experience Series, number 4.) New York: New York University Press, 1986. Pp. xiv, 224. \$35.00.

Graham Russell Hodges has written a masterly study of an important but as yet largely unstudied sector of the American working class. Until the transformation of cities into manufacturing centers in the 1800s, carters comprised the largest single occupational group, one responsible for the movement and collection of all goods within the city. Known for their rowdy and often surly behavior, they were not considered part of the world of the skilled craftsworker, and in 1785 the Mechanics Society denied them admittance. But neither were they part of the world of the unskilled: the slaves, free blacks, and Irish aliens who did the city's menial tasks for subsistence wages.

In harmony with the city government, the cartmen followed what Hodges terms a "corporate philosophy." In return for acceding to municipal regulations including set fees, restrictions on weights and measurements, and controls against forestalling and engrossing (cornering vital supplies), the drivers received a licensed monopoly, which limited their numbers (from 8 in 1617 to 3001 in 1830) and excluded blacks, aliens, and temporary drivers. Moreover, cartmen's sense of independence was enhanced by rules demanding that each carter own his horse and cart and that they neither hire employees nor accept a salary from merchants or storekeepers. They were not among the wealthy (although there were a few exceptions), but they were a proud group of independent proprietors.

The city gave cartmen "freeman" status, entitling them to vote in municipal and assembly races. Al-

though their political weight did not prevent the mayor from calling in licenses when abusive behavior or the flaunting of regulations became prominent, it restrained the city magistrate from severe penalties, brought regular increases in carting fees, and kept merchants from hiring private drivers. Cartmen emerged as a prominent electoral force in the era of the American revolution, when they strayed from the merchant-oriented Delancey party and thus parted from many of their mercantile patrons. Merchants and cartmen reunited in the Federalist party at the inception of the Constitution but again divided in the 1790s, when the carters' sense of personal independence, much bolstered by the revolution, was deeply offended by the attempts of prominent Federalists to coerce their votes. This rejection of deference occurred in tandem with the transfer of allegiance of the majority of the city's artisans.

Cartmen later differed from New York's mechanics in their attitude toward regulation and profit control. They refused to follow the Workingman's party in 1829 since its opposition to regulation and patronage struck at the heart of their trade (many cartmen received offices from the city such as inspectors and measurers). They also did not enter Tammany's fold but preferred the Whig party, which shared their concern over the entry of the Irish into the marketplace. A superb chapter on the fiercely egalitarian, racist, and nativist cartman Isaac Lyon deftly explains the cartmen's position.

Greater attention might have been paid to cartmen's relations with the community of skilled craftsworker. Too, the attempt to cover such a large time period in a relatively short book occasionally makes for abrupt transitions. But this should not detract from Hodges's accomplishment. Working on one of the most obscure sectors of the working population and evincing fine skill in tapping many sources, he has produced a carefully crafted and finely written, subtle work. No historian of early American social history will want to miss it.

HOWARD B. ROCK  
Florida International University

MARYLYNN SALMON. *Women and the Law of Property in Early America*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1986. Pp. xvii, 267.

Marylynn Salmon provides a comparative look at women's legal status in America from colonization through the early national period. She investigates conveyancing and contract law to compare the control of wives and husbands over family property; divorce, separate maintenance, and alimony agree-

ments to show concepts of the marital bond and attitudes toward the standard of living deemed acceptable for women residing apart from their husbands; separate estates to assess the degree of independence allowed propertied women; inheritance law to determine the property rights of women of all classes; and provisions for widows to define the status of women within the family. Salmon concludes that women's legal position varied dramatically between colonies and that the differences were primarily ideological, that wives had limited proprietary capacity but female autonomy gradually expanded against a background of women's enforced dependence, and that these changes were shaped by the interplay of economic and demographic factors with social attitudes.

Salmon suggests that Puritan lawmakers refused married women protection for their estates because to do so would have undercut the unity of married persons by acknowledging conflicting interests between husband and wife. Seeking clear and efficient land management, a simplification of property rules, and the elimination of conflicting interests within the family, New Englanders revised laws on conveyancing, dower rights, and separate estates. They created a social and legal system to enforce patriarchal authority and family interdependence. Because marriage was considered a civil, not a sacred, contract, absolute divorce (*a vinculo matrimonii*) was possible in dire circumstances, but women in proper families had no need to protect themselves for husbands and children would care for them out of love and duty. Wives in New England had no property rights until the second quarter of the eighteenth century.

Maryland, New York, Virginia, and South Carolina closely followed English rules on matters of land conveyancing, private examinations, dower, and separate estates. Because legislators believed that the possibility of coercion made financial transactions by wives suspect, the law in these colonies acted for the "good" of women by removing their contractual capacity and protecting their separate interests.

These differing approaches were embodied in the elimination of chancery courts in Connecticut, Massachusetts, and Pennsylvania. Because chanceries administered equity law, their presence or absence determined a colony's position on female separate estates. Puritans and Quakers disliked chancery for its time-consuming and expensive procedures, its reliance on the independent judgment of a chancellor rather than a jury, and its association with the prerogative powers of the king or governor.

Significant as was ideology, Salmon recognizes the importance of economics and demography. Southern men died early, leaving young widows with minor children to support. Dower was extended to

personal as well as real property in the Chesapeake. New England wives, widowed at a later age, could look to grown children for financial assistance. "Reform" of dower in New England meant that widows received dower only in the real property husbands possessed at death; in Pennsylvania it meant that all debts were paid before dower was granted. By the end of the eighteenth century, tenants in dower faced an uncongenial economic and legal environment in which their interests were seen as hindering economic development and as detrimental to the goals of heirs and creditors.

Salmon takes on two historiographic issues—that the legal status of women improved in the move from the old to the new world and that progress was reversed in the early national period until the "revolutionary" acts governing married women's property were passed in 1848. She finds that colonial departures from English law were not always advantageous to wives who, ironically, may have fared better in jurisdictions that most closely replicated English law. She further states that, although colonial women did not experience significant improvement in legal rights after the American revolution and certain postrevolutionary developments resulted in losses, many more marked women's increased autonomy. The reforms of the mid-nineteenth century were more evolutionary than revolutionary, the result of gradual changes in the regulation of separate estates.

Salmon establishes the parameters of this subject and provides an important interpretation that future work will test. She offers new insights into the interdependence of legal and social change and the complexities of measuring women's status. Although Salmon provides fascinating readings of specific cases, we need further studies in probate and civil proceedings to understand the interaction of custom with statute and the impact of statute on the lives of various female groups. As women took management of separate estates from trustees, what changes were made? Did widows seek cash settlements of dower, and to what use did they put them? How did southern and especially black mistresses fare in inheritance? In family disputes, did judges give equal treatment to spinsters and their brothers? Such questions are of interest to women's and social historians, and Salmon's fine work provides a method and a context for their further exploration.

LEE CHAMBERS-SCHILLER  
University of Colorado

LEE VIRGINIA CHAMBERS-SCHILLER. *Liberty, A Better Husband—Single Women in America: The Generations of 1780–1840*. New Haven: Yale University Press. 1984. Pp. x, 285. \$22.50.

This insightful study explores the ramifications of spinsterhood in nineteenth-century America by drawing on the writings of northeastern women born between 1780 and 1840. In particular, it probes the aspirations, fears, successes, and failures of women who consciously rejected marriage because they envisioned liberty as a better husband. Spinsterhood emerges in this account not so much as a corollary of demographic shifts and economic changes, although such factors are duly noted, but rather as a matter of cultural transformation and individual volition. In the heyday of the cult of domesticity, some women chose "singlehood" for economic autonomy, intellectual growth, physical privacy, and personal power. As Lee Virginia Chambers-Schiller points out, their quest is historically significant because it reflected a distinctly feminine response to romantic individualism and because it provided subsequent generations of women with a viable alternative to marriage.

The liberty that these women associated with single status eluded them more often than not. Although economic and vocational limitations comprised one set of barriers, ideological limitations were sometimes more formidable. According to Chambers-Schiller, "the cult of single blessedness" emerged and thrived in the nineteenth century precisely because it was confluent with the cult of domesticity. A single woman could be a true woman through her self-abnegation and service to others; she continued to be defined by her familial relationships, devoting time and energy to the needs of parents, siblings, and kin. Nonetheless, as this sensitive analysis suggests, the quest for personal power through spinsterhood was inherently subversive, an ambitious challenge to prevailing gender roles cast in submissively feminine guise. Consequently, difficulties evolved as much from women's internal conflicts as from external constraints. For example, in a society that sanctified motherhood, menopause could prove especially stressful; in contrast to her married counterpart, who may have felt pride and relief at having finally fulfilled her biological destiny, the single woman sometimes experienced self-doubt and depression at the foreclosure of her biological destiny.

The author identifies a backlash against spinsterhood in the last third of the nineteenth century. A tendency to see the willingness of women to live outside the control of men as an attack on male dominance and to dwell on its sexual implications transformed spinsterhood into a social problem. By the early twentieth century women found it ever harder to invest single status with the positive attributes that antebellum women had attached to it. In some ways, attitudes toward spinsterhood, which began with early colonial equations of singleness



with sin, came full circle after a significantly liberating interlude.

Because the women under consideration in this study came of age largely in the antebellum era, the analysis of the postbellum period is not supported by a comparable depth of research, nor is the periodization always clear. Also, given the diversity of the sources, which include fiction, poetry, and didactic literature, as well as letters and diaries, some statement about the problems and possibilities they present as historical evidence would have been useful. Nevertheless, Chambers-Schiller has used her sources with skill and imagination to create an important work of scholarship. Not only has she drawn on and incorporated the familiar paradigms in women's history but she has reshaped them into a new paradigm that enriches our understanding.

NORMA BASCH  
Rutgers University,  
Newark

HERMAN J. VIOLA and CAROLYN MARGOLIS, editors.  
*Magnificent Voyagers: The U.S. Exploring Expedition, 1838–1842*. Washington, D.C.: Smithsonian Institution Press. 1985. Pp. 303.

The Smithsonian Institution's exhibition commemorating the Wilkes Exploring Expedition was a notable success. So, too, is this beautifully illustrated volume of essays written by fifteen specialists to illuminate the impact of the expedition on American science. The book sketches the history of the expedition and in appendixes details the vessels, chronology, and landings. The five studies on the effect of the expedition on American science are the most valuable. They demonstrate the great impact that the observations and specimens collected by scientists accompanying the expedition had on botany, zoology, geology, and anthropology. Other accounts describe the vessels of the squadron and their modification for the service, explain methods of surveying and manufacturing charts, and describe Charles Wilkes's naval career. A final pair of essays are important contributions to cultural history since they describe the storing and display of the expedition's collections at the National Gallery in the Patent Office and their subsequent reluctant acceptance by the Smithsonian Institution.

Herman J. Viola and Carolyn Margolis have edited a physically attractive, beautifully printed volume with superb illustrations, mostly of artifacts and art from the expedition, and a valuable group of essays that contribute substantially to our understanding of the cultural and scientific history of nineteenth-century America.

K. JACK BAUER  
Rensselaer Polytechnic Institute

THOMAS R. HIETALA. *Manifest Design: Anxious Aggrandizement in Late Jacksonian America*. Ithaca, N.Y.: Cornell University Press. 1985. Pp. xiv, 284. \$27.50.

In this disapproving study of expansionism associated with Manifest Destiny in the 1840s, Thomas R. Hietala sets forth the thesis that territorial aggrandizement was essentially defensive in purpose. It arose, he says, from Americans' "uneasiness about race relations, modernization, population growth, antislavery agitation, and international competition for colonies and trade" (p. xi). Put another way, "the expansion of the Tyler-Polk years, like that of the 1890s, grew largely out of a recurring domestic malaise that found expression in American aggrandizement" (p. 270). This interpretation is familiar as applied to expansionism in the late 1890s, but it has not commonly been used for the earlier period.

This thesis presents several difficulties. The author sometimes has to select his evidence carefully to support it. He is right in saying that many southerners sought the annexation of Texas to provide more slave territory and that some northerners accepted Robert J. Walker's rationalization that inclusion of Texas in the Union would drain off southern slaves. However, a great many more northerners opposed that particular expansion precisely out of fear of increasing slave territory and proslavery voting strength in Congress. Hietala devotes much attention to Walker's famous pamphlet but none to the many opponents of the pamphlet, who made this point among others.

Some of the illustrative cases do not fit the thesis very well. The desire for trade and markets, especially in the Pacific, explains only part of the expansionist impulse in the Far West. (Here as in other places Hietala dips into earlier monographic writing, as all synthesists must, but he is not always careful to acknowledge his borrowing.) Some parts of his book are already familiar as explanations for an aggressive interpretation of Manifest Destiny—for example, the chapter on American exceptionalism is largely the approach of Albert K. Weinberg, with new illustrations added. It is not convincing evidence for a defensive thesis. Other sections seem to have little bearing on the thesis or on foreign relations at all. The discussion of modernization (that is, that Americans yearned for simpler, preindustrial times) works in a few cases, such as that of Thomas Ritchie, but most of the argument drifts into purely domestic history. Finally, the book does not cover all important cases of expansionism and omits the desire to annex Ontario and the Maine boundary question. Perhaps these do not fit the thesis as well as the West and Southwest.

Hietala has stated his thesis so categorically that he cannot bend and compromise as the evidence requires. Certainly, when Americans have been



boastfully self-confident in foreign relations they have often been motivated by two complementary impulses: to bluff the opponent and to convince themselves that they are indeed all they say they are. But the application of this idea to the 1840s would require much more finesse than Hietala has shown. If readers can take his overstated thesis with a grain of salt, they will find many useful passages in the book, such as his analysis of John C. Calhoun's anti-imperialism, his explanation of the frontier and naval policies of Tyler and Polk, and his account of Polk's political troubles during the second half of his term.

DAVID M. PLETCHER  
Indiana University

K. JACK BAUER. *Zachary Taylor: Soldier, Planter, Statesman of the Old Southwest*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1985. Pp. xxiv, 348. \$29.95.

Although he was both a war hero and a president of the United States, Zachary Taylor is a distinctly secondary figure in American history. He was a national military and political figure for less than five years, and most of his military career before 1846 was uninteresting and undistinguished. Born in 1784, Taylor grew up in Kentucky and spent a long military career on frontier assignments before emerging as the first hero of the Mexican War. His popularity catapulted him to the presidency in 1848, but he served only sixteen months before dying in July 1850.

This interpretive biography by K. Jack Bauer is the first scholarly study of Old Rough and Ready in more than three decades. Bauer's purpose is not to rewrite Holman Hamilton's detailed two-volume account but to trace "those parts of Taylor's life that shaped his later actions" as a general and a president by isolating the most significant "experiences and ideas" in his life (p. xxiii-xxiv). The author's task was complicated because most of Taylor's personal papers were destroyed in the Civil War. In addition, Taylor was not a reflective or inquisitive man, and he did not think or write extensively about the issues of his day. Moreover, much of Taylor's career was "unexciting . . . mundane and boring" (p. xxii). In spite of these obstacles, Bauer has produced an excellent biography of a second-tier figure. The book is well written, and the extensive research is based on a combination of primary sources and secondary studies. Bauer has partially compensated for the lack of Taylor's personal papers by using other manuscript collections including several small holdings of Taylor papers.

In his assessment of Taylor, the author draws well-reasoned and balanced judgments based on his

sources and on his own considerable expertise on the period of the Mexican War. Bauer gives Taylor high marks as a frontier commander and praises Taylor's handling of the Texas border crisis in 1846, but he is critical of Taylor's conduct of the actual war in northern Mexico, particularly the Monterrey campaign. Bauer also acknowledges Taylor's naiveté, ignorance, and stubbornness as a politician. President Taylor's antisouthern positions on several issues and his intolerance of secessionist attitudes are attributed to his strong sense of nationalism and long service in the army. Although he was poorly prepared for the presidency and did not develop a coherent domestic or foreign policy, Taylor matured as president, according to Bauer, and was on the verge of significantly reshaping his administration and the Whig party at the time of his death.

Zachary Taylor emerges in this study as a man of conservative instincts and limited intellectual capacity—a man who viewed his military and political responsibilities in narrow and unimaginative terms. His personality was marked by stubbornness, petulance, and a strong temper, but Taylor also succeeded in developing a "nearly impenetrable mask" that made him difficult to understand. This fact ultimately prevents Bauer from fully comprehending his subject and forces him to conclude that Old Rough and Ready was an enigma to contemporary observers as well as modern historians.

JOHN H. SCHROEDER  
University of Wisconsin,  
Milwaukee

THOMAS H. O'CONNOR. *Fitzpatrick's Boston, 1846-1866: John Bernard Fitzpatrick, Third Bishop of Boston*. Boston: Northeastern University Press. 1984. Pp. xvi, 292. \$22.95.

As a subject of historical research, episcopal biography has matured from the denominational hagiography of years ago to its contemporary form as a critical focal point for exploring larger historical themes. Such is the case with Thomas H. O'Connor's study. O'Connor uses the career of John B. Fitzpatrick, the third Roman Catholic bishop of Boston, to assess mid-nineteenth-century Boston in its struggle with a growing foreign (that is, Irish Catholic) population and the impending crisis of disunion and civil war. According to O'Connor, Fitzpatrick should be remembered as the pivotal figure who bridged the cultural and political gap between the brahmin establishment and the Irish underclass during the tumultuous period from 1846 to 1866. Fitzpatrick's personal bearing, education, and public demeanor won him the trust and respect of Boston's Protestant leadership and made possible the bishop's mediation of the potentially

explosive conflict between the city's Anglo-Saxon, Protestant elite and the immigrant, Irish Catholic community. Fitzpatrick's legacy, says O'Connor, was the gradual, nonviolent integration of Irish Catholics into the social and political life of Boston in accord with America's constitutional principles.

O'Connor reviews in a readable fashion the main themes of the era—Irish immigration to the United States because of the potato famine, nativist fears and violence sparked by the foreign menace, the crisis of union triggered by slavery's expansion into the territories following the Mexican War. His biographical sketch of Bishop Fitzpatrick provides new information about the man himself as well as the development of the institutional church in antebellum New England. As a lens through which to examine pre-Civil War Boston, however, this episcopal biography is more tantalizing than substantive.

The reader is told, often, that Fitzpatrick enjoyed influential relations with Boston's upper class, including Lemuel Shaw, Rufus Choate, Abbott Lawrence, and Robert Winthrop. As a member of the "Thursday Evening Club," Boston's "Who's Who" of the time, Fitzpatrick moved freely among the ruling Whig elite. The reader wants to know the specifics of these relationships and how they translated into power and influence. If Charles Sumner was Fitzpatrick's "good friend" (p. 60), what was the product of this friendship with respect to abolitionism? If Fitzpatrick had "warm and friendly personal relations" with President Franklin Pierce (p. 98), did the bishop exercise influence in some way regarding the slavery controversy? Were Fitzpatrick's connections with Massachusetts Senators Sumner and Edward Everett effective in quelling the nativist assaults on Boston Catholics during the mid-1850s?

O'Connor's study suggests some fascinating lines of inquiry regarding the development of American Catholicism. One example must suffice. Fitzpatrick accepted the basic idea of a pluralistic society. To him, Catholicism was not in conflict with U.S. constitutionalism. Moreover, he staunchly insisted on the Americanization of his immigrant parishioners. Even in the face of Vatican pressure and criticism from fellow bishops who declared him "not sound on the school question" (p. 117), Fitzpatrick refused to develop a Catholic school system in the Boston diocese. In light of his long relationship with Orestes Brownson, and to a lesser extent Issac Hecker, does Fitzpatrick's career shed light on the development of a distinctly American Catholicism? If Fitzpatrick shaped Boston, might not Boston have influenced the bishop as well? Given the current Vatican effort to rein in the American church on matters of ecclesiastical authority and discipline, it is useful to recall the story of Roman Catholicism in America and how each has been shaped and defined by the

other. This was true for Fitzpatrick and Boston; it remains true today.

SAUL E. BRONDER

*International Brotherhood of Teamsters*  
Washington, D.C.

PRISCILLA J. BREWER. *Shaker Communities, Shaker Lives*. Hanover, N.H.: University Press of New England. 1986. Pp. xviii, 273. \$24.95.

This is the first major attempt to apply demographics to the study of Shakerism. Priscilla J. Brewer sets out to describe Shaker experience in the nineteenth century. She is especially concerned with success and "failure." She has compiled statistics on the Church Family at New Lebanon, New York (headquarters of the movement) and comparative data on the eleven eastern Shaker communities to which she confines her study. The appendixes may be the most valuable part of the volume. Her interpretation of the data and, particularly, the assumptions informing her views on the decline of the Shakers are more problematical.

Brewer explains early Shaker success as a by-product of the religious energies generated by evangelical revivals. She sees the failure of the movement as the result of a complex web of factors, including the departure of large numbers of young members on reaching maturity, a decline in the available pool of leaders because of high rates of apostasy, and an increasing percentage of women as the century passed. For Brewer these quantitative factors point to more basic qualitative problems such as the inability of the society to educate and socialize its youthful members, the ineptitude of its leaders after 1821, and the deleterious effects of hired men who performed essential tasks in the communities. In her view these and other nonstatistical issues caused the society's failure, a process beginning as early as the late 1820s.

Brewer is to be commended for turning to the manuscript resources available for the study of Shakerism as a partial corrective to the distortions caused by the overemphasis on material artifacts. These documents, including U.S. census schedules, provide statistics and alert her to important neglected topics. Brewer is especially instructive on the place of extended families in the recruitment of new members, the care of displaced and deprived children by the society, and the circumscribed role of women among the Shakers. Her description of the changing fortunes of Grahamism is also suggestive. None of these themes, however, depend directly on demographic data.

Much of Brewer's argument, unfortunately, rests on a subjective and troublesome reading of anecdotal texts. For example, objective criteria cannot be

established for identifying "lukewarm Shakers" or measuring "spiritual torpor"—major categories for her. Brewer cites isolated cases of mental illness, suicide, fornication, and pregnancy to imply more general patterns. She assumes that apostasy automatically undermined the commitment of the faithful; in fact, it frequently had the opposite effect. Further, material prosperity did not lead ineluctably to spiritual apathy; the Shaker work ethic was religiously motivated. She shows too little understanding of the symbolic value of the "gifts" during the era of manifestations and calls them "outrageous." Finally, the decision to limit the study to the eastern villages was ill advised because expansion into the West drained eastern Shakerism of leaders, financial resources, and psychological energy—all relevant to Brewer's thesis.

STEPHEN J. STEIN  
Indiana University

COLLEEN MCDANNELL. *The Christian Home in Victorian America, 1840–1900*. (Religion in North America.) Bloomington: Indiana University Press. Pp. xvii. 193. \$25.00.

This book is the first publication in a new series, "Religion in North America." The intent of the series is to explore "the religious world of the average person." Colleen McDannell describes the relationship between American Christianity and the home. Prior attention to this subject has stopped short of her claim that Victorian "beliefs in the divine nature of the home created a particular form of American popular piety" (p. xiii).

McDannell's book is noteworthy for her bold thesis as well as for her interdisciplinary approach to religious studies. On the basis of a comparison of Catholic and Protestant forms of domestic piety, she concludes that three forms of religion in America exist alongside each other: denominational, civil, and domestic. Her thesis is that "by combining traditional religious symbols with a set of middle-class domestic values the Victorians rooted their home virtues in the eternal and allowed the more abstract traditional symbols to assume a real presence in everyday life. . . . To understand the Christianity of this period we must look not only at public symbols of civil religion or particular theologies, but at the sacramental character of the home" (p. 151).

McDannell has done for domestic religion what Robert Bellah did for civil religion. But her comparative method is more convincing than the evidence offered in support of the claims about civil religion. In claiming that the family served as a middle ground between the community and the self, she has done as much as possible to reconstruct the influence of the "abstract theology" of priests and

pastors, denominational and church publications, popular literature, novels, and private journals on religious practices in the home.

A good example of her use of the many sources available to the historian of religion is a chapter on domestic architecture and the Protestant spirit. In a fascinating description of the connection of morality and religion with the purchase and maintenance of "a Christian home," she indicates how the Christian home served as a sacred symbol. In this context she then describes the function of parents, especially mothers, as priests and teachers of religion in the home.

This book casts more illumination on the experience of Protestants in America than it does on that of Roman Catholics. The thesis is developed on the assumption that between 1840 and 1900 Protestant and Catholic Americans displayed commonality related to the hope for a Christian America and the rise of the middle class. Consequently, only Irish Catholics who moved up into the middle class are discussed. Despite this limitation, the book has broken new ground in the comparison of Catholic and Protestant popular piety of the period.

McDannell draws a conclusion from her comparison of Catholic and Protestant domestic piety that could lead to revisions of American Protestant church history. "Since Protestantism did not demand the 'real' presence of God for its ritual, the home could function as a center for devotion. The Bible defined Protestant worship, and it could be read easily in the home setting. Protestant home worship equalled the church service, and many believed it actually was more effective. . . . The church acted as an extension of the family" (p. 104). If she is correct, this functional reversal of the nature of church and family deserves additional study. This thesis could be extended and explored as it relates to twentieth-century domestic ideology, family worship, and patterns of leadership in churches.

McDannell writes with disarming simplicity, yet her work is scholarly in method and citation. An extensive bibliography is included. The absence of academic jargon and argumentation makes this book attractive as a text for undergraduate as well as graduate students of religion and culture in America.

JANET FORSYTHE FISHBURN  
Drew University

JOHN W. KUYKENDALL. *Southern Enterprise: The Work of National Evangelical Societies in the Antebellum South*. (Contributions to the Study of Religion, number 7.) Westport, Conn.: Greenwood. 1982. Pp. xv, 188. \$25.00.

A study of the evangelical tradition of regenerating American life, not by individuals or denominations but by national voluntary societies, is a tall order for a short book, especially one in which almost 20 percent of the text is block quotations, and over 40 percent of the pages are elongated quotations and scholarly apparatus. That John W. Kuykendall succeeds as well as he does is testimony to a strong scholarly composure and a purposeful chronological narrative.

The attempt by the "big five" national voluntary societies to establish a benevolent empire in the South is portrayed as a failure. Bearing a heavy load of moral ambition and a rarefied millennial vision, "some of America's earliest traveling salesmen" sought to strew the South knee-deep in Bibles, tracts, Sunday schools, and missionaries. The South resisted, lobbing in the face of Yankee agents every excuse imaginable (and some objects much harder): happy impiety and ignorance, untrained and offensive personnel, antimissionary sentiment (Baptists), and domination by Calvinists (Methodists) and abolitionists (slaveholders). The heroes of this story were the Presbyterians, who supplied the labor, the money, and the momentum to the southern segment of the national strategy to grow the kingdom of God in the American garden.

Early failures did not dampen the spirits of the big five, but the economic panic and ecclesiastical schism of 1837 did. Some of the societies withdrew from the South. The three that remained kept going, but the cost was high. "No offense" became the reigning motto with regard to southern views on slavery as the spine of evangelicalism's social morality had been broken. The "fragile prosperity" of 1845 to 1865 resulted from fiscal security, sectional schisms, which ironically forced southern denominations to turn the remaining societies into almost denominational agencies, and new strategies of program development such as colportage (which rendered evangelical mobilization indigenous).

In the end, however, the Evangelical United Front failed, and at one point the author wonders whether it ever really existed. Unable to work on two scales at once, the front functioned easily on the monumental, awkwardly on the particular. It could not survive the friction between denominations and failed to surmount sectional animosities. The embargo of the "southern enterprise" on social issues sadly twisted the evangelical career from its true path.

The volume has many virtues, not the least of which is to break our preoccupation with the western focus of these societies. The South also mattered. The book's most damaging shortcoming is its insufficient discussion of how much the South mattered. Given the meager resources the big five pumped into the South and the marginal role the

South played in the millennial mentality, the last chapter of the book might have easily been framed in terms of success rather than failure. By not dredging deeper into the background and motivation of the agents or into the type of southern support these agents received, Kuykendall's study lacks sufficient depth of analysis. Because Kuykendall does not give shadow and significance to the formation of voluntary societies in many cities without agents and ignores the role of women in southern benevolence, we still do not have a full picture of their influence in the region, and the discussion is disfigured by a constricted angle of vision. And because the study does not consider that the big five occupied only a small niche in the evangelical effort in the South and in many ways were as sauce to the real dish, the amplitude and uniqueness of southern religion are cramped.

LEONARD I. SWEET

*United Theological Seminary*

MICHAEL O'BRIEN and DAVID MOLTKE-HANSEN, editors. *Intellectual Life in Antebellum Charleston*. Knoxville: University of Tennessee Press. 1986. Pp. xiv, 468. \$45.00.

Containing contributions by a dozen different scholars with a dozen different perspectives, this volume has the weakness of almost all such works: it sprawls out in many directions and still has gaps. In the preface Michael O'Brien laments the silence on subjects such as science and theology, and I regret that some of the recent work on Charleston's free mulatto elite by Michael P. Johnson and James L. Roark is not included.

Still, O'Brien's preface and especially David Moltke-Hansen's first chapter indicate a central theme that runs like a thin, frayed thread through the rather disparate fabric of this volume. After the revolution Charleston fell behind booming northern cities such as Philadelphia, New York, and Boston, but even so its intellectual life grew steadily. A sluggish economy and the continued enslavement of half of the population created constraints, but the proud old city entered the Victorian age long before the Civil War. This book succeeds in reminding historians that the cultural and intellectual domination of the Northeast in the last antebellum decades did not exclude real progress in cities in the hinterlands.

The rest of the book pursues this general theme with varying degrees of consistency and success. Arthur H. Shaffer unfolds the career of David Ramsay, the Pennsylvania-born and Princeton-educated historian who stressed nationalism in his famous works, although his adopted city was beginning to lean in the opposite direction even before his

death in 1815. Mark D. Kaplanoff describes the political career of high-living coastal planter Charles Pinckney, who swung away from Ramsay's optimistic nationalism and in 1821 delivered one of the strongest defenses of southern rights during the bitter congressional debate over Missouri statehood. Michael O'Brien analyzes awkward, ambitious Hugh Legaré, who was loyal to his adopted city but increasingly alienated from it too. A successful lawyer and brilliant scholar, he supported the Union during the nullification crisis and was swamped by John C. Calhoun and his followers. Lacy Ford examines the career of James Louis Petigru, an upcountryman who joined the Charleston elite. Like Legaré he had a love-hate relationship with Charleston. He too held for the Union and lost election after election, but he became a successful lawyer and made many cultural contributions to the city such as helping establish the South Carolina Historical Society. John McCardell describes William Gilmore Simms's relentless effort to make a living as a writer and his attempts to blend the new world of work and business with the old world of wealth and ancestry in Charleston, a difficult job that helped cause Simms to share Legaré's and Petigru's mixed feelings about the city. Layton W. Jordan presents Christopher Gustavus Memminger, an immigrant orphan who made good in Charleston. He championed railroads, better sanitation, public education, and other modern reforms successfully enough to indicate that antebellum Charleston was not as far out of the national mainstream as it has often been portrayed.

The volume then shifts from luminaries to sensibilities and a slightly broader approach. Jane H. Pease and William H. Pease discuss the nonconfrontational, "rounded edges" style of discourse that developed in the wake of the tensions created by Denmark Vesey's insurrection and the nullification crisis; the bitter dispute over medical education in the city was the exception that proved the rule. Charles Joyner describes the creole Gullah language and Afro-Christianity of coastal plantation slaves who held on to some of their old tribal cultures. Here would have been the place for a counterbalancing contribution by Johnson and Roark describing Charleston's free mulatto elite, who took a very different path, consciously rejecting their African heritage and copying the white elite as much as possible. Theodore Rosengarten details the tenuous existence of John D. Legaré's journal, *The Southern Agriculturist*, which championed scientific farming. Steven M. Stowe examines the contrasting positions of upper-class women in Charleston and on the plantation in the writings of Caroline Howard Gilman, Susan Petigru King, and Mary Boykin Chesnut. Finally, Richard Lounsbury describes at

too great a length the use and misuse of the classics by some of the Charleston elite.

This volume is by no means a complete study of the life of the mind in antebellum Charleston, but scholars will find it a well-done series of glimpses—an effective reminder that, on the eve of the Civil War, the old city had sparkle as well as fire.

F. N. BONEY

*University of Georgia*

JAMES OSCAR FARMER, JR. *The Metaphysical Confederacy: James Henley Thornwell and the Synthesis of Southern Values*. Macon, Ga.: Mercer University Press. 1986. Pp. 295. \$28.95.

Although the past decade has brought a marked growth of interest in the intellectual history of the Old South, no extended scholarly treatment of James Henley Thornwell has preceded James Oscar Farmer, Jr.'s welcome study. Few individuals in the antebellum period matched Thornwell in insight and influence, for, as a leading Presbyterian and South Carolina educator, he had ready-made access to the region's elite.

Farmer's book traces the major events in Thornwell's life but is less a biography than a study of the man's thought and milieu. "A metaphysical confederacy . . ." Farmer asserts, "had to precede the physical confederacy" (p. 16), and Thornwell, he believes, played a critical role in forging this southern system of belief. Born in upcountry South Carolina in 1812, the son of an overseer, Thornwell's extraordinary mental gifts were early recognized by members of the local gentry, who supported his education at local schools and then South Carolina College. A spiritual awakening after graduation directed him toward the ministry, and he enrolled in Andover Seminary. The New England winter prompted his return to the less rigorous climate of South Carolina, where he was ordained. Like a number of other prominent Carolinians of his generation, Thornwell married above his station, gaining not just a wife but wealth, slaves, a plantation, and social status. In 1837 he returned to South Carolina College as a professor and a decade later became president of the institution. In 1855 he joined the faculty of nearby Columbia Theological Seminary and soon took on the editorship of the *Southern Presbyterian Review*, one of the region's most prominent periodicals. Fighting throughout the 1850s to avert secession, Thornwell nonetheless served as an articulate defender of slavery and ultimately became a champion of the new Confederacy and the separate Southern Presbyterian Assembly created late in 1861. But he did not live to see the fate of the new nation, for he died in the summer of 1862, not quite fifty years old.



The embodiment of southern conservatism and orthodoxy, Thornwell believed, as he himself put it, that "innovation ought always to be avoided" (p. 217). A leading advocate of the doctrine of the spirituality of the church, Thornwell insisted that Scripture must dictate not only religious beliefs but also ecclesiastical organization and polity. Thus, he opposed departures from tradition as varied as the use of instrumental church music and ecclesiastical interference with slavery, which he insisted was an exclusively civil matter. Yet, at the same time, he recognized the need to reconcile the nineteenth century's new scientific spirit with religion, and he devoted much of his scholarship to that end.

Without the efforts of clergymen such as Thornwell, Farmer concludes, "the righteous spirit that drove the Confederacy in its early years could not have been so strong" (p. 271). But, like a number of recent students of late antebellum southern thought, Farmer finds paradox and contradiction behind the seemingly "high level" of regional "confidence and optimism" (p. 287). The evangelicism underlying southern beliefs generated a humility beneath the region's apparent arrogance; the South sought to defend tradition and faith in the modernistic terms of science and rationality; the region retreated from the threat of change by inaugurating a revolution, a "radical rationalist act" (p. 289). Farmer's success in placing Thornwell within a wider context of southern society and belief will make his book required reading for all seeking to understand the mind of the South in secession and Civil War.

DREW GILPIN FAUST  
University of Pennsylvania

RICHARD E. BERINGER *et al.* *Why the South Lost the Civil War*. Athens: University of Georgia Press. 1986. Pp. xi, 582. \$29.95.

This book addresses one of the persistent controversies in Civil War historiography. In it Richard E. Beringer, Herman Hattaway, Archer Jones, and William N. Still, Jr., critically examine the reasons most frequently advanced for Southern defeat and offer an alternative explanation.

Drawing on their own research as well as the findings of many other scholars, the authors persuasively argue that Northern superiority in numbers did not account for the defeat of the Confederacy. The contention that more advanced Northern economic development insured victory neglects, they say, that "amazing, even magnificent, achievement—the industrialization of the Confederate South" (p. 216). The Union blockade, which remained largely ineffective, did not bring about the collapse of the Confederacy. The familiar argument

of Frank L. Owsley that the Confederacy died of states' rights they, in effect, turn on its head. Independent-minded Southern governors such as Joseph E. Brown of Georgia and Zebulon B. Vance of North Carolina may have annoyed Confederate authorities, but their activities in raising state troops and in caring for their constituents in the Confederate service "permitted the energies and resources of the states to contribute significantly to the Confederate war effort" (p. 457). The authors also challenge the theory recently advanced by Grady McWhiney and Perry D. Jamieson, in *Attack and Die* (1982), that Southern defeat was the consequence of Confederate reliance on the frontal charge on the battlefield. The casualty figures advanced by McWhiney and Jamieson, they argue, have been misinterpreted: "A thorough analysis of the numbers does not substantiate the thesis that a Confederate mistake in assuming the tactical offensive caused them to murder themselves in bold and repeated attacks" (p. 456).

In its negative aspects, then, this book is well informed, up to date, and mostly convincing. Its persuasiveness is the greater because the authors, in challenging the findings of other historians, display not one hint of bad temper or superiority in their analysis.

Rather less effective is their positive argument that the defeat of the Confederacy was the result of "the weakness of southern nationalism," which had "very shallow foundations" and was constantly undermined by Southerners' feelings of guilt over slavery. "The epitaph on the Confederacy's tombstone," they conclude, "should read, 'Died of Guilt and Failure of Will'" (p. 34).

This thesis presents difficulties. In part it seems tautological: Southerners lost the Civil War because they did not want to fight any longer. Moreover, talk of Confederates' guilt about slavery rests on limited evidence; Southerners seldom expressed moral discomfort with the peculiar institution, and historians who have worked carefully in the Southern records, such as Eugene D. Genovese and James L. Roark, do not believe that these sporadic utterances indicate a widespread attitude.

The thesis of the book also fails to persuade because of the manner in which it is argued. Abandoning their original plan to present "a series of short, discrete essays" that would examine a number of causal explanations, the authors chose instead to prepare "an extended, integrated discussion," which becomes in places a kind of general history of the Civil War. Analysis of the reasons for Southern defeat is obscured in a narrative of military and political events that mostly repeats material in *How the North Won*, the excellent military history of the Civil War that two of the authors published in 1983.

An admirable critique of familiar interpretations and an impressive synthesis of new scholarship, this book will not, its authors realize, settle the controversy over the causes of Confederate defeat, but it will, as they hope, "stimulate fresh thought and . . . move future discussion onto more complex ground."

DAVID HERBERT DONALD  
Harvard University

JOHN DAVID SMITH. *An Old Creed for the New South: Proslavery Ideology and Historiography, 1865–1918*. (Contributions in Afro-American and African Studies, number 89.) Westport, Conn.: Greenwood. 1985. Pp. ix, 314. \$35.00.

John David Smith has written a compact, carefully and thoroughly researched, useful study in historiography. After surveying the racial thought of postbellum white America, most of which was decidedly proslavery, Smith incisively examines the "Nationalist Historians," whose writings dominated American historiography from the 1880s through the 1920s. Hermann von Holst, James Ford Rhodes, Albert Bushnell Hart, James C. Ballagh, Philip A. Bruce, and U. B. Phillips disagreed over slavery, but they were ardent nationalists when they wrote about the past. Holst, a Russian refugee trained in scientific history in Germany, decried the denial of the slave's civil rights, whereas Rhodes, Hart, and John Back McMaster softened or rejected any moral criticisms of slavery, particularly any that veered away from the assumptions of white supremacy. To Rhodes and Hart, whose influence was widespread, slavery was wrong because it created sectionalism and in turn brought on the Civil War. Their strictures, however mild (and they were far too mild for Holst and blacks such as W. E. B. Du Bois), were too radical for proslavery scholars such as Bruce, Ballagh, and Woodrow Wilson. In 1918 Phillips's proslavery classic, *American Negro Slavery* swept the field and dominated American scholarship for the next several decades.

Smith's knowledge is vast and well ordered. He has obviously searched exhaustively through the primary sources and read widely in the secondary literature. But little in his book, either in subject matter or argument, is unfamiliar or unacceptable to historians. Further, his tendency to summarize and be encyclopedic—to list everything from obscure dissertations to barely remembered monographs—does not serve him well. More analysis of the sort Smith provides in his sections on Rhodes, Hart, and Ballagh would help considerably. Unfortunately, Smith's long chapter on Phillips turns out to be an unimaginative survey of *American Negro Slavery*.

Smith effectively shows contemporary scholars how much pioneering (if flawed) work their predecessors did. Hart's *Slavery and Abolition, 1831–1841* (1906) started the scholarly discussion of slave medicine; Ballagh's best-known work, *A History of Slavery in Virginia* (1902) explored the origins of slavery: "Years before Oscar Handlin, Carl N. Degler, and Winthrop Jordan battled over this issue, Ballagh already had staked out the boundaries of the debate" (p. 147). In *Democracy in the South before the Civil War* (1905) Gustavus W. Dyer of Vanderbilt University pointed to the significance of the yeoman farmers and foreshadowed Frank L. Owsley's later and more thoroughly developed argument. Such contentions constitute a valuable subtheme in Smith's book. This, combined with the book's storehouse of facts and extensive documentation, makes for a useful study.

BRUCE CLAYTON  
Allegheny College

GAVIN WRIGHT. *Old South, New South: Revolutions in the Southern Economy Since the Civil War*. New York: Basic. 1986. Pp. x, 321. \$19.95.

"Where did this New South come from, and what became of the Old?" asks Gavin Wright in this book. By "New South" he means the South since 1940. His "Old South" stretches back to the Civil War, a conflict, he contends, that not only created a free labor force in the region but also changed southern "laborlords" into "landlords," with significant consequences.

Before the war, he argues, the bulk of southern capital was in slaves, movable assets that rendered the owners indifferent to internal improvements, town building, and industrialization. Thus, the southern economy diverged sharply from that of the North, where wealth was in land, and capitalists, eager to enhance their investments, built canals and railroads, encouraged the growth of cities, and supported manufacturing. After the war southern capitalists, stripped of their slaves and with their principal investment in land, moved toward the mainstream of American economic life. But it was to be almost a century before the South was absorbed into the the national economy.

Wright's thesis is that the labor market of the post-Civil War South remained regional until World War II and that it was this regional market that "made all the difference"—or, at least, most of it. An important legacy of slavery was a low agricultural wage that depressed all wages in the South, made it unattractive to workers from elsewhere, and, together with planter hostility to outside influences and to improvements in local education, effectively segregated the region from the national

labor market. Given a low wage and world demand for its products, the South could concentrate on unmechanized staple agriculture, particularly cotton growing. True, some industries developed, chiefly textiles, but, hampered by shortage of capital, unskilled management, lack of a technological community, workers unused to industrial discipline, and poor quality of its product, it took even that industry until the 1920s to destroy the hegemony of the New England mills, and manufacturing as a whole did little to raise the standard of living in the South. What, then, did bring the South into the American mainstream?

According to Wright, the process began in the 1930s, when New Deal farm programs undermined plantation tenancy and other federal legislation forced an increase in southern industrial wages. These changes, reinforced by the demands of the wartime economy in the 1940s, induced mass out-migration, agricultural mechanization, and the breakup of the mill village. In addition, the South's rich sources of energy, federal investment in the region, and latterly, a good business climate there have contributed to the disappearance of the old southern economy. No single-minded economic determinist, Wright also gives credit to the civil rights movement of the 1960s for bringing the South into synchronization with the rest of America.

This necessarily brief outline of Wright's book does less than justice to the sophistication of his argument. Wright is that rare scholar who combines a mastery of economics with a deep knowledge of history and a concern to address a wide spectrum of readers. His prose is graceful, direct, and free of jargon. Statistics, charts, and graphs abound, but as addenda to the text. A sequel to Wright's *The Political Economy of the Cotton South* (1978), this book is an amplification and revision of the author's conclusions in the earlier work. A brilliant addition to the literature on the South since Appomattox, Wright's work will long be analyzed, its conclusions debated and doubtless modified. All historians will want to read and ponder this book.

IRENE D. NEU  
Indiana University

GERALD DAVID JAYNES. *Branches Without Roots: Genesis of the Black Working Class in the American South, 1862–1882*. New York: Oxford University Press. 1986. Pp. ix, 351. \$29.95.

Gerald David Jaynes makes a strong argument that the scarcity of credit resulting from the abolition of capital in slaves was a crucial determinant of the South's postbellum economy. The southern economy might have developed differently had the northern victor broken up the plantation system or

come forward with a "Marshall program" of economic aid or credit relief. But sentiment for reform took a back seat to calls for restoring the plantation-dominated cotton export economy. A massive infusion of economic aid similar to that provided to war-torn Europe in the 1940s was simply out of the question in this laissez-faire society. The consequence for the South was a dearth of credit that in turn gave rise to the baneful institutions of sharecropping and the crop lien.

If this book did no more than show that the South's underdeveloped capital markets were preconditions for economic failure, it would be a very good book, indeed. But Jaynes accomplishes much more by detailing the decentralizing processes that led to sharecropping in the cotton economy—ironically, when the economic tendency elsewhere was toward centralization. Jaynes brings to bear a close familiarity with both modern and premodern forms of labor organization. He has read widely in the literature on slavery and emancipation and mined the exhaustive records of the Freedmen's Bureau. Economic shrewdness is mixed with common sense. His book thus carries authority when it argues that payments in shares, not wages, prevailed in the South immediately following emancipation, that the gang system first gave way to the smaller squad system before eventually devolving into tenantry, and that economic problems generally associated with collective work patterns and deferred payment systems explain the final emergence of sharecropping.

Jaynes provides insights into the factors affecting the participation of black women in the work forces, the relative inefficiency of plantations organized on shares, and the black work ethic. Jaynes also contributes importantly to the debate over whether the southern labor market was "competitive." He recasts the question by asking if it was "free" and answers, "No government which allows its laboring population to mortgage its labor by enforcing debt peonage can claim to have free labor" (pp. 313–14). This formulation will hardly settle the debate, but it should certainly enrich it.

The book is not without shortcomings. Too often Jaynes fails to indicate his position in a debate until several chapters have elapsed, leaving the reader to deduce it in the interim from internal cues. The book also pays insufficient attention to the floating pool of casual day labor—mainly young, unmarried black males—that several recent studies show figured prominently in the mature postbellum economy. Moreover, historians presently exploring the interconnections between politics and economics in the postemancipation South will fault the book's economic determinist approach. Despite Jaynes's occasional criticisms of the work of modern neoclassical economic historians, he seems to share their

premise that market forces alone dictated the emergence of sharecropping following the Civil War.

This is nonetheless a very good book, maybe a seminal one in the history of the black working class in the South during its early years of freedom. It deserves the widest possible readership.

LAWRENCE N. POWELL  
Tulane University

RICHARD JULES OESTREICHER. *Solidarity and Fragmentation: Working People and Class Consciousness in Detroit, 1875–1900*. (The Working Class in American History.) Urbana: University of Illinois Press. 1986. Pp. xix, 263. \$24.95.

In 1886 some Detroit workers were clandestinely drilling with Winchester rifles on the outskirts of the city, preparing for the revolution that was in the air. Larger numbers were out in the streets: more than six thousand struck on May Day; ten thousand—one quarter of the labor force—marched on Labor Day. But this moment “when people believed almost anything was possible” quickly faded. Why? For Richard Jules Oestreicher, the answer is embodied in the title of his book. “Social change in Detroit,” he writes in this important study of that city’s working people, “produced intertwining tendencies toward class solidarity and fragmentation, both tendencies simultaneous outgrowths of the consequences of industrialization and urbanization” (p. xviii).

Oestreicher locates working-class fragmentation partly in the diverse conditions of work in industrializing Detroit but even more in the distinct ethnic communities of native whites, Germans, Poles, and Irish that provided the fundamental source of identity for most of the city’s workers. The core of Oestreicher’s book, however—and its most original contribution—is devoted to showing how the city’s workers were able briefly to transcend these (largely ethnic) barriers and to build in the 1880s a common “subculture of opposition.” This subculture embraced a network of institutions (from Knights of Labor assemblies, to cooperative stores, to singing societies, to a labor party) as well as “a wide range of informal practices and commonly understood moral precepts”—cooperation, mutual trust, equality, and mutual assistance (p. 62).

The first eight months of 1886 changed the subculture of opposition from a minority culture to a mass culture, as membership in the Knights of Labor grew eight times and total union membership jumped by more than 250 percent. The “summer of possibilities,” which ran from May Day to Labor Day in 1886, shook many Detroit workers “loose from their traditional loyalties and normal patterns of daily life” and seemed to promise a fundamental realignment of class, culture, and power in the city

(p. 166). But, in even less time than it took the spirit of solidarity to coalesce, the forces of fragmentation (which had never entirely disappeared) reasserted themselves—along with a counterattack by employers. Within a year the labor movement lost five thousand members; before the decade was over most of the other institutions of the subculture of opposition were gone, although its legacy continued to shape class relations in subsequent years.

Oestreicher’s well-written and extensively researched book is an outstanding study of the Knights of Labor as well as one of the very best of the growing number of community studies of working-class life that are gradually transforming our understanding of class and community in industrializing America. The main unanswered question takes us outside of Detroit: how and why did the dialectic of solidarity and fragmentation vary across the American industrial landscape? Oestreicher makes some brief gestures toward answering this important question. But much more comparative and synthetic analysis is needed if we are to begin to reassemble the temporal and geographic slices of working-class history that have been so carefully uncovered by Oestreicher and other recent labor historians.

ROY ROSENZWEIG  
George Mason University

WILLIAM ISSEL and ROBERT W. CHERNY. *San Francisco, 1865–1932: Politics, Power, and Urban Development*. Berkeley and Los Angeles: University of California Press. 1986. Pp. xiv, 294. \$35.00.

In seeking to explore the relationship “among politics, power, and urban development” (p. 1), this book makes an important contribution to our understanding of American cities and the Bay Area in particular. The discussions of the theoretical evolution of the field of urban political studies are a contribution in themselves. The book has many other strengths as well. Instead of limiting their study to a short span of time, William Issel and Robert W. Cherny take in a broad sweep of sixty years. They also avoid the pitfall of narrowness by including significant discussions of San Francisco’s culture, its ethnic and class patterns, its economy and business life, and its labor history. In addition, like much of the best recent writing on city politics, this book is thoroughly interdisciplinary. Finally, the book is very informative, even to persons who fancy themselves well informed on this city’s history. The authors have obviously done a massive amount of research in the original sources, and they have worked the result into an excellent narrative of city politics.



Interpretively, the book raises many questions. The authors argue that recent urban political historiography has been dominated by pluralist, elitist, and Marxist interpretations, and they tend toward the elitist and Marxist views. They hold that, in San Francisco politics, the critical power to initiate was largely in the hands of the business community. Although rejecting the more traditional elitist and Marxist models of politics, the authors rest their case close to these models by concluding that "in this larger sense, then, power was not shared but concentrated" (p. 211). This monopoly, in turn, eventually led to a city "polarized" on class lines (p. 186).

Conflict between labor and management frequently tore at the fabric of San Francisco politics, but, even by the authors' own statistics, the city does not appear to have been consistently polarized from 1901, or even 1916, to 1932. In a partisan sense, the city was often 80 percent Republican, so, class struggle, if any, took place within a bourgeois, striving group. But none seems to have been going on. By drawing an arbitrary line in the middle of the voting spectrum, the authors argue that the city was divided into two camps. In fact, if one rearranges these ward data in numerical percentage order, the distribution of percentage follows a normal curve. Opposite extremes certainly existed in the city's politics, but the community apparently was spread between them, rather than polarized around them.

The narrative also assumes a unanimity of capitalist interest that the analysis does not prove. Although the authors reject a booster interpretation of the city's politics on the grounds that boosters had complex interests that sometimes transcended the competition between cities, they ignore the complexity of class interests—in such issues as the construction of the bay bridges—that often set members of the same class against each other. Further, the political struggles that the authors record often did not have much to do with urban development. The major struggles of the era when polarization is supposed to have occurred grew out of labor-management issues, not city-building ones. The principal story that the book tells is one of labor-management conflict, which is only partially related to the objective of investigating the relationships "among politics, powers, and urban development." Labor history is important, but it cannot be taken as fully or even largely representative of the broader stream of city politics.

ROGER W. LOTCHIN  
*University of North Carolina*

C. ROBERT HAYWOOD. *Trails South: The Wagon-Road Economy in the Dodge City-Panhandle Region*. Norman: University of Oklahoma Press. 1986. Pp. xv, 312. \$19.95.

In this book C. Robert Haywood gives a picture of the opening of a number of minor, though important, trails south of Dodge City that penetrated into southwestern Kansas, western Oklahoma Territory, and the Texas Panhandle. Haywood presents much interesting detail on the opening of the various trails and on their evolution and use. He tells of the great variety of goods and supplies that passed along the trails for army posts, for military campaigns against the Plains Indians, and for ranches, farms, and the developing towns and communities. The Dodge City-Fort Supply Trail, its extension to Fort Elliott and on to Mobeetie and Tascosa, and the Jones and Plummer Trail, so often overlooked by writers on western trails, are discussed at length.

In gathering material for the story of the trails, Haywood traversed many of the routes on foot, but whether he did so for a part or all of a trail he does not say. He claims he thus gained a feeling for the trails and for the people who made them. The maps add to an understanding of the routes, but Haywood has failed to label the Fort Elliott Extension of the Dodge City-Fort Supply Trail, the Adobe Walls Trail, and several of the smaller trails and has omitted many of the names of towns, creeks, crossings, sod way stations, stage stations, inns, road ranches, and stockades. Although he discusses the effects of the coming of the railroads on the trails and the early towns, none of the maps show the routes of the several railroad lines that traversed the region.

Haywood gives a good picture of life and accommodations along the trails, of the hardships and dangers encountered by people moving west, of mail contractors and of how the mail was delivered, and of the freighters, stage drivers, and the operators of a number of way stations. Many thumbnail biographies are included. It was a rugged life, full of adventure, excitement, heroism, and danger.

Haywood's story might have been better organized to avoid some needless repetitions and contradictions. For instance, on pages 97 and 112, we find the same phrase: "When weekly deliveries by hack were authorized to Tascosa via the Crooked Creek post office and the Ohio settlement at Pearlette." One also has difficulty drawing conclusions about the importance of the Jones and Plummer Trail from these statements: "For two decades of the 1870s and 1880s, the Trail served as a major freight-highway" (p. 65); "The Jones and Plummer Trail was not heavily used" (p. 78); and the Jones and Plummer Trail was famous as a cattle trail leading from the South to Dodge City (see page 96 for this conclusion). Missing from the index are a number of names of persons, places, and geographic features mentioned in the text.



Scholars in western history will find this book interesting and informative, containing new knowledge; the general reader will be overwhelmed by details.

JOSEPH MILTON NANCE  
Texas A&M University

CHARLES J. ROONEY, JR. *Dreams and Visions: A Study of American Utopias, 1865–1917*. (Contributions in American Studies, number 77.) Westport, Conn.: Greenwood. 1985. Pp. xi, 209. \$29.95.

In 1961 Charles J. Rooney, Jr., received a list of 362 titles that were potentially works of utopian literature, compiled by the late Lisle A. Rose. After a year's reading Rooney identified "at least 100" authors of "true utopian works" (p. x). These works, 119 in all, are the subject of *Dreams and Visions: A Study of American Utopias, 1865–1917*, although, indicative of Rooney's generally casual employment of numbers, the earliest was published in 1869. Had he consulted other bibliographies, he would have discovered many more "true" utopias in this period.

Rooney devotes much of this book to classifying these utopias. He identifies social problems they address and their solutions. He also places the utopias on a societal spectrum ranging from individualism to protototalitarianism and summarizes their attitudes toward equality, morality, work, and progress.

The book is weakest in explaining the motivations for utopianism and the complex relationship between the literature and the ideational and sociocultural context in which it was generated. In this regard, it can hardly compare with the rich analyses in Kenneth M. Roemer's *The Obsolete Necessity: America in Utopian Writings, 1888–1900* (1976) and Jean Pfaelzer's *The Utopian Novel in America, 1886–1896* (1984). Rooney is certainly correct in attributing this idealistic outpouring of fictional societies to their authors' belief that their vision of the good, middle-class life was threatened by industrialization and urbanization. Beyond this, his historical explanations are less than satisfactory. For example, by asserting that utopians were "a product of eighteenth-century thought" (p. 77), he stresses their debt to Enlightenment ideas and overlooks important nineteenth-century influences such as romanticism. Moreover, he is oblivious to scholarship in the not-so-new social and cultural history. This is understandable, since his bibliography includes no secondary studies on American history published later than the early 1960s, shortly after he finished cutting Rose's list from 362 to 119 titles.

ALLAN M. AXELRAD  
California State University,  
Fullerton

DAVID L. ANDERSON. *Imperialism and Idealism: American Diplomats in China, 1861–1898*. Bloomington: Indiana University Press. 1985. Pp. ix, 237. \$24.95.

This book is a significant addition to the already large historical literature on American policy in China in the late nineteenth and early twentieth centuries. Without ignoring the perspective in Washington, David L. Anderson focuses his attention on the eight men who served as American minister in Peking between 1868 and 1898, on the grounds that policy was more often determined in the field. The result is a fascinating study of ambiguity and disjointedness, idealism and self-interest in American policy over the period. As such, it presents a notable challenge to works that have discerned more continuity in American goals and tactics.

The parameters of American China policy are nicely illustrated by the first two American diplomats studied: Anson Burlingame and J. Ross Browne. Burlingame, the most idealistic of the American representatives in Peking during the late nineteenth century, recoiled against Western bigotry and coercion of China. Although he wanted China to become more like the West, he sought a gradual transformation in full cooperation with Chinese authorities. China would be treated as an equal. The United States would cooperate with the European powers, but only if they too pursued a policy of forbearance toward China. Browne shared his predecessor's hope to reform China but disagreed with his gradualist, cooperative approach. China had opened its doors to the West only under severe pressure, and internal reform would come about only if the Western powers, acting together, insisted on it, with force if necessary. The six diplomats subsequently assigned to Peking pursued policies somewhere between these two extremes.

Among the best accounts are those that deal with John Russell Young and Charles Denby. Young, who served only briefly in China, wrestled with fundamental questions and attempted to develop an independent American policy toward China that would reconcile American ideals and interests. Denby, who served for twelve years under both Republican and Democratic administrations, often found himself at odds with the State Department, which disapproved of his close cooperation with other Western powers and his activist role on behalf of the few Americans who wanted to invest in China.

A final chapter skillfully examines John Hay's Open Door notes. Anderson traces the notes' origins and effectively argues that they reflected Hay's well-considered views. Hay's diplomacy, he argues convincingly, "was a practical attempt to make the best of a bad situation" (p. 186). The notes reflected the

ambiguities of American policy developed over the previous four decades and "blended masterfully the ideals and self interests of the United States" (p. 171).

The book is based on solid research in personal papers and American government documents. Research in foreign archives, such as British Foreign Office records, as well as in American missionary collections, probably would have strengthened the presentation at some points. But, given the book's focus, this is not a fundamental flaw.

This well-crafted book offers a stimulating interpretation based on good research. It is, in sum, an impressive achievement.

KENTON J. CLYMER  
University of Texas,  
El Paso

KENTON J. CLYMER. *Protestant Missionaries in the Philippines, 1898-1916: An Inquiry into the American Colonial Mentality*. Urbana: University of Illinois Press. 1986. Pp. xi, 267. \$28.95.

Kenton J. Clymer's inquiry into the American colonial mentality has yielded a splendid study, in which a complex picture of Protestant denominationalism in a colonial setting emerges. Drawing on extensive research in the archival sources, Clymer writes lucidly of the tangled and changeable relationships of Protestant missionaries with each other, with the Filipino peoples, and with the other varieties of American colonialists—military personnel, civilian government functionaries, and the business community. Assuming that the missionaries played a crucial role—as allies and critics of the government—in shaping American colonialism, Clymer explores missionary attitudes toward a range of topics including Filipino culture, Roman Catholicism, Filipino nationalism, and American colonial policy and practice. The book will serve students of Philippine history and of American imperialism as an indispensable companion to Peter W. Stanley's *A Nation in the Making* (1974); together the two volumes offer a comprehensive account of American involvement in the Philippines in the years before World War I.

Valuable as this study is in its elucidation of the missionary role too often slighted and misunderstood in discussions of American colonialism, perhaps greater significance lies in its detailed depiction of the politics of turn-of-the-century American Protestantism. The access that missionary and denominational leaders had to the highest levels of American decision making, both in Manila and in Washington, indicates the hegemonic position that Protestantism occupied in American culture and certainly bolsters Clymer's contention that Ameri-

ca's colonial mentality cannot be fully understood without examining the missionary mind. Most strikingly, however, this study displays a badly divided Protestantism, unable to take a united stand on anything but opposition to gambling at the cockpit. The intra- and interdenominational squabbling over territorial rights and doctrinal questions that undermined comity agreements and threatened co-operative ventures on the mission field replicated divisions among Protestants at home. Roman Catholics in the Philippines and in the United States had less to fear from Protestant colonialism than they imagined.

Clymer admits that measuring the lasting impact of Protestant missionaries in the Philippines is difficult; as he tells their story, their failures to meet their goals for conversions and to live up to their agreements for comity seem to outweigh their limited successes. In contrast to other mission fields where educational ventures at least had a lasting effect, educational experimentation in the Philippines was largely in the hands of the American colonial government, which maintained, under pressure from American Catholics, a policy of religious neutrality. But, if, paradoxically, American missionaries may sometimes have had a greater impact in countries where America's was not the colonial presence, the Filipino missions were not such anomalies as to prevent this study from providing a model for work that needs to be done on other major mission fields. This book offers the best available account of how America's vast foreign missionary enterprise operated in the field. More studies of American Protestantism in isolation, as it were, in other cultures will help us locate the fault lines along which the structures of Protestant hegemony collapsed.

PATRICIA R. HILL  
Wesleyan University

MITCHELL OKUN. *Fair Play in the Marketplace: The First Battle for Pure Food and Drugs*. DeKalb: Northern Illinois University Press. 1986. Pp. xii, 345. \$27.50.

Focusing on the controversies surrounding the adulteration of food and drugs during the nineteenth century, Mitchell Okun has attempted a study of what he calls "the first battle for pure food and drugs." Although dates cited in this volume range from 1820 to 1886, the author seems principally concerned with the period from 1865 to 1886. He acknowledges the growing complexity of a rapidly urbanizing post-Civil War America and notes the trend toward the formation of associations and professions. The antiadulteration movement of the 1880s, he contends, involved an alliance of businessmen, chemists, physicians, and pharmacists, whose

objectives in turn helped shape the performance of the sanitarians who occupied the enforcement boards. Examining the relationships between these groups makes this book a history of social and institutional change as well as public health.

Okun chronicles events pertaining to the control of food and drugs that were played out in Washington, D.C., New Jersey, and Massachusetts, but he reserves special emphasis for New York. He is generally convincing when he argues that most of the controversies that surrounded the federal law of 1906 had already surfaced in New York by 1886; he borders on the simplistic, however, when he concludes that "nothing really changed" from the 1880s to 1906 "except that the tendencies and technological complexities of modern commerce already present in New York had spread to encompass the rest of the country" (p. 295). The appearance of such personalities as Harvey W. Wiley and Robert McDowell Allen, the influential work of scientists at state agricultural stations, the effective lobbying of the Association of American Dairy, Food and Drug Officials (the organization's name changed several times), and the growth and dissemination of scientific knowledge especially in bacteriology all influenced public health legislation after 1886.

Most of the available publications pertaining to adulteration of food and drugs and efforts to effect control deal with the late nineteenth century or the twentieth century. Concentrating on developments that antedated the Progressive era, Okun's study makes a contribution to American public health history and scholarly understanding of the regulation of food and drugs. Therein lies the strength of this book. Unfortunately, it also has its weaknesses. Although Okun's research seems adequate and he presents interesting data, he does not tell his story advantageously. The text lacks the vitality of related work by such historians as Oscar E. Anderson, Jr., and James Harvey Young. Readers are left to make their way through a laborious presentation. The author introduces a large cast of characters and offers little assistance in sorting out their relevance. The basic chapter outline, as well as internal organization within chapters, leaves a great deal to be desired. Consequently, the reader must hurdle across one chasm after another. The businessmen, charlatans, and experts—the distinctions blur—are fully presented, but one is never quite sure if the struggle over food and drugs described by this author was waged in the national, regional, state, or municipal arena.

MARGARET RIPLEY WOLFE  
*East Tennessee State University*

VICTORIA A. HARDEN. *Inventing the NIH: Federal Biomedical Research Policy, 1887–1937*. Baltimore: Johns

Hopkins University Press. 1986. Pp. xiii, 274. \$32.50.

It is difficult to realize that the National Institutes of Health (NIH), which dominate biological research in this generation, date only from the 1930s. This book recounts the gradual change in the attitude toward scientific research and the role of the government that led Congress to create the NIH. Victoria A. Harden begins her history with the background leading to the establishment of the Hygienic Laboratory in 1887. She shows how Dr. John Shaw Billings and advocates of a national health agency clashed with the United States Marine Hospital Service and indirectly pushed it into establishing the laboratory. The Hygienic Laboratory, which had little concern for the wider interest of public health, sought to keep all government health programs under its jurisdiction. In the process, it fought every effort to create a national health department until well into the twentieth century.

The years 1890 to 1940 witnessed a change in the ingrained American opposition to using tax money to improve human health—as opposed to the health of domestic animals and birds. This period also saw growing awareness of the value of scientific research. Stimulated by its work in connection with the Spanish-American War, the Pure Food and Drug Act of 1906, and World War I, the Hygienic Laboratory slowly expanded. At the end of World War I Charles Holmes Herty and a group of chemists interested in pharmaceuticals conceived of creating a national biomedical research institute. Herty and his associates assumed the funding would come from private sources. When this assumption proved false, they turned to Senator Joseph E. Ransdell of Louisiana and the federal government.

Ransdell had served in the Senate since 1912 and actively supported a series of public health measures. In 1926 he introduced a bill into Congress to create a National Institute of Health to study the "fundamental diseases." In this same year the Parker bill to reorganize the Public Health Service was introduced. Harden details the four years of sparring between various interest groups and the political machinations leading to the eventual enactment of both laws in 1930. A final chapter surveys developments in the NIH since 1937.

The NIH is fortunate in its historian. She knows her sources, is familiar with virtually all the secondary literature, and places the early history of the NIH in its proper economic and political context. And incidentally she has done a marvelous job of showing the intricacies of maneuvering bills through Congress.

JOHN DUFFY,  
EMERITUS  
*University of Maryland*

JAMES LIVINGSTON. *Origins of the Federal Reserve System: Money, Class, and Corporate Capitalism, 1890–1913*. Ithaca, N.Y.: Cornell University Press. 1986. Pp. 250. \$27.50.

When President Woodrow Wilson signed the Federal Reserve Act in December 1913, America established for the first time an institution specifically charged with acting as concertmaster to the economy. Such a move had been discussed during the long depression of 1873–93 and again following the panic of 1907, by which time a consensus had been reached. In view of Robert Craig West's excellent study a decade ago, one can quite properly ask what this carefully researched and well-written interpretation contributes.

James Livingston contends that "the origins of the Federal Reserve System lies in the awakening and articulation of capitalist class consciousness" (p. 18). Opening with a critique of the literature, he supplies a version of the late nineteenth-century economy that is startling in its novelty but, perhaps as a result, unconvincing.

In his effort to place the formation of the Federal Reserve in context, Livingston includes a chapter on the development of large-scale enterprise. Whether Alfred D. Chandler, Jr., would recognize and accept the version of *The Visible Hand* (1977) that Livingston employs is questionable. Surely it is more plausible to contend that the evolution of managerial enterprise was accomplished more by vertical integration, innovative administrative techniques, mechanization, and economies of scale than by intentional antiunion activity.

According to Livingston, the key question is: "Can the origins of the Federal Reserve System be understood apart from the emergence of a modern ruling class" (p. 226)? Chandler is cited—no doubt much to his surprise—in support of Livingston's negative answer. Furthermore, this query only gives rise to another. What about the contention that the ruling class in America in 1913 could look backward on substantial economic growth, both absolutely and relatively, since the end of the previous central bank institution?

The connection between the author's ample historical evidence and his ideology is too tenuous to prevail. Since other industrial countries already had central banks and still had to cope with the business cycle, nonideological explanations are not only possible but also preferable. Once economic reality dictated the foundation of a central bank, the disagreements among the players concerned form rather than substance, and, therefore, reliance on ideology is rendered superfluous. This is not to reject the author's view that a ruling class existed (and exists); nevertheless, he has not demonstrated that the concept of a ruling class is necessary to

understand the origins of the Federal Reserve System. Finally, two hypothetical questions come to mind. Has the record of the Federal Reserve System fulfilled the expectation of the ruling class? Does the author propose to abolish the Federal Reserve System, and, if so, would his purpose be to thwart the ruling class?

SAUL ENGELBOURG  
Boston University

WAYNE A. WIEGAND. *The Politics of an Emerging Profession: The American Library Association, 1876–1917*. (Contributions in Librarianship and Information Science, number 56.) New York: Greenwood. 1986. Pp. xi, 322. \$39.95.

Based on many previously unexamined archival sources, this is the first detailed scholarly study of the political and historical development of the American Library Association (ALA) from its formation in 1876 until World War I. Although Wayne A. Wiegand avoids comparison of librarianship with other feminized areas of work, his description of the unique configuration of one profession is an important addition to the general literature on professionalization.

Some of the findings that have been suggested before in library histories are now meticulously confirmed: the inordinate influence of Melvil Dewey on professional development, the librarians' half-hearted effort to dictate literary standards, and the rivalry between the new library-school graduates and the librarians trained by traditional apprenticeship. More surprising is the story of the bitter and protracted struggle for control of the ALA. In conflict were the proprietors of the large urban northeastern libraries and the librarians of large and small institutions in the West and Midwest.

The battle to "democratize" the electoral machinery of the ALA was not strictly class-based. The author demonstrates the essential homogeneity of ALA leaders during this period. Members of the executive board in the mid-1880s were most likely to be married, middle-aged, white, Anglo-Saxon, Protestant males who were directors of research libraries located in the Northeast. They were the sons of managerial fathers and without formal library-science training. Thirty years later, aside from the increase in representation of women on the board to 33 percent, the leadership was much the same, although several alterations are worthy of note. Members were now somewhat more likely to have professional fathers, to be born and educated outside the Northeast, to have attended library school, and to direct northeastern, midwestern, or southern public libraries.

ALA infighting was essentially a contest between eastern conservatives—with their patronizing attitude toward the public and anyone living west of the Alleghenies—and more democratically minded liberals, who were more diverse in their geographical base. Until the late 1890s the controversy centered around Dewey and his commitment to the technocratic practicalities of librarianship. Dewey courted the heads of small libraries, chiefly women, to build his winning political coalition. Dewey had lost his influence by the early 1900s, partly because the ALA bureaucracy had developed sufficiently so that other males—building a new coalition that appealed to regional resentments and the electoral majority of women—were willing to spend as much time as Dewey once had to shape the agenda of the national professional association.

This study of the effort to accommodate shifting pressures and power groups within library leadership can serve as a model for histories of other professional associations. The author examines the political motivations behind the votes and speeches and skillfully blends theoretical assumptions, empirical data, and historical analysis.

DEE GARRISON  
Rutgers University

STEVEN M. BUECHLER. *The Transformation of the Woman Suffrage Movement: The Case of Illinois, 1850–1920*. (Douglass Series on Women's Lives and the Meaning of Gender.) New Brunswick, N.J.: Rutgers University Press, 1986. Pp. xvi, 258. \$32.00.

In 1913 Illinois women gained partial suffrage, including presidential voting rights, from the state legislature. A year earlier popular referenda for woman suffrage had been defeated in Ohio, Michigan, and Wisconsin. The victory in Illinois demonstrated that all was not lost in the Midwest and reinvigorated the pursuit of partial voting rights as an important strategy in the offensive to gain political equality. We have not had nearly enough history of midwestern suffrage politics, and sociologist Steven M. Buechler's book will help fill that gap.

Buechler hopes to address some of the larger questions regarding women's political movements raised in works by Aileen Kraditor, William O'Neill, and Ellen DuBois. (He considers these authors to be "noteworthy exceptions" to the general run of suffrage historians, who usually "fail to offer an interpretation of the events they describe" [pp. 19–20].) He is particularly interested in how political movements are transformed, that is, how they change their "ideology, tactics, strategies, social base, organizational forms, and resources" (p. 19). He also considers why suffrage for women, which began as a radical demand in 1850, became a fairly trivial

change by 1920. But his focus is so narrow and is examined in such a broad conceptual framework that historians may find his conclusions overly simplistic. His decision not to compare his findings on Illinois with recent work on other state-level suffrage campaigns must have been conscious but also seems curious and even somewhat arrogant. The implication is that historians are mere antiquarians whereas only sociologists have insights into such lofty topics as "movement transformation."

Nonetheless, historians will find much that is worth reading here. Buechler divides the suffrage movement in Illinois into three periods. Between 1850 and 1870 women's suffrage was perceived by both its proponents and its opponents as a radical demand, which threatened to undermine the separate spheres of men and women. Most of the leaders of the suffrage movement were from what Buechler calls the "old middle class," daughters of farmers, merchants, and ministers. The realization that all women were potential victims because of the economic and legal dependency of marriage gave suffrage a universal appeal that had the potential for crossing class lines. From 1870 to 1890 Buechler shows that the growing anxiety over urban dislocation, class conflict, and immigration made the leaders of the suffrage movement in Illinois, still largely from the old middle class, far less able to cast their arguments to include working-class women. The irony was that the early successes of the movement—increased control over wages, custody of children after divorce, and education—set middle-class women apart and made suffrage far more meaningful to them than to other women. Suffragists were tempted to argue that winning the vote would not jeopardize the feminine sphere or the supremacy of the middle class and might even help avoid class conflict by providing more feminine "social control." Suffragists thus became defenders of their class view rather than advocates of expanding liberties for all women. Few of these elements were lost in the final drive for women's suffrage between 1890 and 1920, when arguments of expediency—always used to some extent—became the only possible strategy for uniting what were now irreconcilable class, racial, and ethnic constituencies into one movement. Buechler insists that these arguments were not, however, mere strategies but that "the movement's actual analysis . . . underwent significant changes toward a narrower posture" (p. 55). He documents for Illinois what is being shown to have been true in many other states; that the endorsement of the suffrage movement by upper-class women, along with their thousands of dollars, made the suffrage movement politically possible and even socially acceptable.

This finding suggests that the bitter resentment of many socialist and radical women against the lead-



ership of the suffrage movement was not irrational but in fact reflective of reality. The importance of giving all women the right to vote could not be denied, but the perception that it would do little to change the class structure of American society put the leaders of working-class women in a terrible bind. Meredith Tax has written about this resentment at some length. Buechler's study, along with Tax's, provides a more sophisticated class analysis of the suffrage movement than we have had heretofore.

SHARON HARTMAN STROM  
University of Rhode Island

LINDA D. VANCE. *May Mann Jennings: Florida's Genteel Activist*. Gainesville: University of Florida Press. 1985. Pp. viii, 200. \$15.00.

In a long life that spanned much of the post-Civil War period, May Mann Jennings became "Florida's most impressive and successful female citizen" (p. 140). Working primarily through the women's club movement, which began in the late nineteenth century, Jennings lent her influence and lobbying skills to countless civic causes and social reforms in a career that stretched from the Progressive era through the 1950s.

Born in 1872 to upper-class circumstances in New Jersey, May Mann was raised and educated in Florida, graduating from a Catholic girls' academy in St. Augustine in 1889. While working for her father in Tallahassee, she met William Sherman Jennings, a newcomer from Illinois and a first cousin to William Jennings Bryan. She married Jennings, already a successful young lawyer and politician, in 1891.

William Jennings's election as governor in 1900 inaugurated the Progressive era in Florida and provided May Jennings as first lady the opportunity to lay the groundwork for what Linda D. Vance describes as "the old-girl network," a group of women around the state who through their connections, usually familial, with prominent politicians could exercise influence on public issues. The unifying elements in the network were the desire for reform and common membership in the Florida Federation of Women's Clubs (FFWC), founded in 1895.

When her husband left the governor's office in 1905, Jennings joined the club movement, serving one term as president of the FFWC and chairing important legislative and conservation committees in the organization for the next forty years. She supported causes ranging from public health and child protection legislation to land grants for the Seminole Indians and tick eradication. To use William L. O'Neill's categorization of reformers, she was a social feminist rather than a feminist, that is,

one who put reform of society ahead of woman's rights. Perhaps Jennings's greatest achievements were in conservation and highway beautification. Under her leadership in 1915, the Florida clubwomen were instrumental in preserving part of the Everglades for public use, first as a state facility and in 1947 as Everglades National Park.

In this brief but satisfactory treatment of a notable southern woman, Vance has succeeded in erasing "the generally held belief that Florida women had no history to tell" (p. 145). Although some scholars might wish for greater documentation on certain issues, such as Jennings's friction with other clubwomen, and more extensive detail on the functioning of the old-girl network, we are indebted to the author for drawing attention to a social feminist who deserves recognition and for adding to our understanding and appreciation of the women's club movement as an agency of reform.

PAUL E. FULLER  
Transylvania University

ROBERT BOOTH FOWLER. *Carrie Catt: Feminist Politician*. Boston: Northeastern University Press. 1986. Pp. xx, 226. \$29.95.

Robert Booth Fowler presents a provocative portrait of the suffrage commander and chief strategist whose "winning plan" led to ratification of the Nineteenth Amendment in August 1920. Carrie Chapman Catt's importance as the last president of the National American Woman Suffrage Association (NAWSA) and founder of the league of Women Voters has never been underestimated, nor has her leadership been examined as it is in this solid but unsatisfactory account.

Fowler's readers will share his frustration with sources too scanty to sustain a full-fledged biography. Other than including the intimate friendships of Catt's later years, Fowler gives little data or detail that is not found in *Notable American Women*. He does delineate her childhood on the Iowa frontier, her hard-earned education despite her father's disapproval, and her careers in teaching, journalism, and suffrage.

Fowler specifically rejects the use of psychological methodology to examine the course of Catt's life (1859-1947), but the evidence is suggestive. Although he repeatedly describes her anger and drive for control, he does not explore the roots or ramifications of such behavior, leaving readers to speculate.

Lack of an explicit methodology also undercuts his analysis of Catt's political leadership. Fowler outlines her organizational theory and practice without presenting any standard models of male or female leadership for comparison and without sug-

gesting what made or motivated Catt. She was not a "feminist politician," a term Fowler fails to define. Ever since she punched a schoolyard bully, she had "fought like a man" for what she wanted—equal power for herself and equal rights for women.

Although excluded from the backroom bastions, Catt had the instincts of a machine politician. Through thirty years of intermittent leadership of the NAWSA, she imposed her principles of precinct organization on that undisciplined body. She undercut rivals and then adopted their ideas as her own, including campaigning for a federal amendment, lobbying Congress, and defeating opponents of woman's suffrage at the polls.

Ironies abound. A Machiavellian politician, Catt created the largest nonpartisan organization in the country, the League of Women Voters. A single-minded suffragist, Catt banned partisan electoral activity by the League. A tireless advocate of peace and disarmament, Catt urged suffragists to support World War I and rebuked Jeannette Rankin for her vote against U.S. entry into the war. An apparently devoted wife and affectionate friend, in public Catt was remote, passionless, aloof. A seemingly mild-mannered and modest woman, Catt was driven by ambition, anger, and resentment of men.

Fowler provides few satisfactory explanations for these ambiguities, just solid exposition of the facts. His presentation is straightforward, thorough, and significant. Limited by lack of evidence as this approach is, it is the best available account. Grounded in historiographical authority, Fowler's book fills a void in our understanding of this most important suffrage leader. Fowler's reasonable tone and willingness to honor both sides in the endless debate over who deserves credit for the success of the suffrage movement seem to suggest parallel lessons and political strategies for more recent feminist politicians.

ELISABETH GRIFFITH  
*McLean, Virginia*

THOMAS G. ALEXANDER. *Mormonism in Transition: A History of the Latter-Day Saints, 1890–1930*. Urbana: University of Illinois Press. 1986. \$19.95.

This pioneer effort of treating an array of subjects in depth for these years makes a solid contribution to Mormon history. Of special interest to Latter-day Saints—for it tells in detail an important part of their story for the first time—it will also interest institutional historians for its story of how an organization characterized by church-directed cooperative economic institutions, a one-party political system, polygamy, and separate courts, schools, and militia moved to a pluralistic community in harmony with American political, economic, and social norms

and still maintained and reemphasized its basic unique tenets. The story chronicled here was a remarkable achievement for church leaders.

Thomas G. Alexander and his research assistants have plowed through a large number of primary and secondary writings to produce a splendid monograph. Besides the usual periodical articles, theses and dissertations, contemporary periodicals and papers, Alexander had access to the letterbooks of the First Presidency, correspondence and diaries of a dozen leaders, as well as council minutes and annual reports. All these heretofore largely unavailable sources have been used with skill and show credit to the researchers as well as the custodians of these records.

The nature of these sources helped determine the major value as well as the limitations of the work. A factual chronicle, it is largely concerned with the institution and its administration at the highest level. Although attention is given to changes that have taken place in the tenets of Mormon thought, the leaders predominate, and the account seldom reaches the people in their congregations. Hence, the study frequently provides little or no plot or story, no culminating events of major significance.

The book deals first with the Mormon search for a pluralistic political system and the developing relationship of the church to political parties and the political process and then with the adherence of the church to competitive capitalism. Thereafter, the author treats church administration, education, missions, public relations, and intellectual problems such as evolution, higher criticism of the Bible, and the conflict between emerging individualism and pioneer cooperation.

I wish the completed manuscript had been worked over thoroughly to have the essay tell the occasional not-so-obvious fact and make the account hold together better and move more quickly, thus reducing needless detail, avoiding repetitions, and underscoring major points. Nevertheless, this work adds much to our knowledge and is worthy of a wide readership.

S. GEORGE ELLSWORTH  
*Utah State University*

FRANK TROMMLER and JOSEPH MCVEIGH, editors. *America and the Germans: An Assessment of a Three-Hundred-Year History*. Volume 2, *The Relationship in the Twentieth Century*. Philadelphia: University of Pennsylvania Press. 1985. Pp. xvii, 369. \$19.95.

Of the various rites commemorating the 300th anniversary of the arrival in North America of the thirteen Krefeld families who founded the first German settlement there, the most elaborate was the Tricentennial Conference of German-American

History, Politics, and Culture at the University of Pennsylvania in October 1983. Financed by such diverse groups as the William Penn Foundation, the Fritz Thyssen Stiftung, the United States Information Agency, and the German Marshall Fund and attended by, among others, Vice-President George Bush, F.R.G. President Karl Carstens, thirty members of the Bundestag, and over eighteen hundred representatives of German-American organizations, the conference was a political occasion for displaying and cementing the relatively happy state of recent American-German (meaning West German) relations and a cultural one for refurbishing the image of German-Americans by celebrating the ethnic pluralism of the United States.

The festivities included discussions and papers by scholars in the fields of history, sociology, German, political science, and English, as well as by some journalists. Under the circumstances, however, the audience addressed, and therefore the tone and content of the contributions, varied widely. Whatever that may have meant for the conference, it raises serious problems in the present volume, which offers revised versions of some of the papers presented. An earlier volume, *Immigration, Language, Ethnicity*, had the virtue of a certain thematic unity—or at least trinity. *The Relationship in the Twentieth Century* has only a time frame, and the diversity of its content is compounded by extensively captioned illustrations that are often at variance with the tenor of the surrounding text.

The twenty-four essays are divided into seven sections, ranging from "American-German Relations, 1900–1950" to "Americanism and Mass Culture." Peter Gay's fascinating, but in this context irrelevant, "Freud's America" constitutes a section by itself. Some essays are the fruit of serious research, although, more often than not, fruit that has been offered before by the same authors for a scholarly audience. Among the best in this category are Klaus Schwabe's judicious explication of the American stabilization policy toward Germany in the 1920s, Detlef Junker's persuasive analysis of the ideological character of Franklin D. Roosevelt's confrontation with the Nazis, Charles Maier's economic interpretation of American sponsorship of the Federal Republic, and Wolfram Hanrieder's systematic and informed exposition of German-American relations in the post–World War II period.

The historiographical essays by Hans-Jürgen Schröder and Arnold Offner skillfully combine a look at some of the more recent literature with the presentation of particular—and, not coincidentally, divergent—lines of interpretation. When taken together, they may well constitute the most useful contribution of the volume to current scholarship. The most interesting commentaries, on the other hand, are surely Fritz Stern's astute assessment of

"An Alliance Turned Normal" and Theo Sommer's "Learning to Live With Our Differences," a piece of superior, informed editorializing, which some sponsors of the conference apparently found out of place at an occasion for "mutual embracing."

Of the five essays published in the section on immigration after 1933, Herbert Strauss's on German Jews offers a wealth of helpful data and some interesting and often neglected perspectives. Three of the others, by contrast, focus on various types of intellectuals, not all of them Jews, who gave this migration its unusual character. The findings and the arguments presented in these papers are likely to be of interest to few but the initiated, who will probably have seen them before.

Interspersed throughout the collection are essays of widely varying quality on Henry Luce's often misguided perceptions of Germany, on German-Americanism, on German anti-Americanism, on the hard times experienced by America's Germanists between 1900 and 1925, on the fate of German-Americans in the United States, and on the American image of Germany since 1930.

No editors, however skillful, could have welded these diverse pieces—some scholarly, some journalistic—into a meaningful whole. Frank Trommler and Joseph McVeigh have concentrated, in any case, on producing a handsome volume in keeping with what they refer to as "the symbolism of the Tricentennial celebration." Parts of it can clearly be read with profit by historians, but historians are not, on the whole, its intended audience.

MANFRED JONAS  
Union College

EWA MORAWSKA. *For Bread with Butter: The Life-Worlds of East Central Europeans in Johnstown, Pennsylvania, 1890–1940*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1985. Pp. xvii, 429. \$39.50.

This is a historical and sociological study of peasant immigrants from East Central Europe in Johnstown, Pennsylvania: Croats, Magyars, Poles, Serbs, Slovaks, Slovenians, and Ukrainians (including the Rusyns). Johnstown, a medium-sized industrial city in western Pennsylvania, a "Pittsburgh in miniature," was dominated by the huge Cambria Iron Works, the predecessor of the Bethlehem Steel Company.

After the Great Flood of 1889 and until the 1920s many thousands of Slavs and Magyars arrived in Johnstown. "Demographic pressure and economic necessity" forced them to leave the old country to find employment in American steel mills, iron works, and coal mines as unskilled workers. All better jobs were held by Germans, Irish, and Welsh.

The newcomers hoped to find "bread with butter"—the life they had missed in Europe. Called Slavish and Hunkies by the oldtimers, they faced an uphill struggle for survival and gradually established thriving ethnic communities. Eventually, many of the immigrants became quite successful.

Three major themes run through Ewa Morawska's book: the intermixture of tradition and modernity in the adaptation of immigrants to American urban industrial society; the formation of the American working class and the proletarianization of the immigrants; the considerable tensions, pains, and suffering they endured in adjusting to an insecure urban industrial life. Chapters 1 and 2 depict the European rural background around 1900. Chapters 3 and 5 describe "the dominant structural and cultural 'framework'" of immigrants from 1900 to the 1930s. Chapters 4 and 6 examine their life-worlds during the same period. Chapter 7 is devoted to internal social stratification of immigrant communities. The concluding chapter analyzes the second generation.

The study reminds us of the classic by William I. Thomas and Florian Znaniecki, *The Polish Peasant in Europe and America* (1918–20) on which Morawska relies perhaps too extensively. She relies too little on Emily G. Balch's standard work, *Our Slavic Fellow Citizens* (1910), which introduced the Slavs to America.

The vast amount of notes covers ninety-five pages with hundreds of sources and additional information to complement the narrative. Regrettably the book lacks a bibliography, which would help the reader lost in the maze of notes. The huge documentation is supplemented by twenty-eight tables in the text and twenty-one in an appendix, twenty-nine interesting and valuable illustrations, and three maps.

A few inaccuracies mar the text. Kordun is in Croatia, until 1918 an autonomous province of Austria-Hungary. "The village of Centinje in Cernagora" should be properly identified as Cetinje, the capital of the Kingdom of Montenegro. Vaso Gajšević was an Orthodox Croatian, not a Serb. Most of the Serbs the author mentions came from Croatia, some from the Vojvodina (southern Hungary), and very few from the Kingdom of Serbia.

In my opinion the author underestimated the political causes of Slavic migrations as well as the heavy price the immigrants paid in blood: deaths and injuries in industrial accidents and disasters should have been more emphasized.

An immigrant herself, the author has produced after four years of meticulous research a definitive study of Slavs and Magyars in a part of industrial America. In spite of some flaws it is a painstaking, detailed analysis, a very good scholarly monograph, and a work of love. It should be included in all

better libraries. It is not easy reading and as such is a difficult book for the general public.

GEORGE J. FRPIĆ

*John Carroll University*

ROBERT A. SLAYTON, *Back of the Yards: The Making of a Local Democracy*. Chicago: University of Chicago Press, 1986. Pp. xiv, 278. \$22.00.

Few urban neighborhoods in industrial America have attracted so much attention during the past century as Back of the Yards, an ethnic working-class community to the west of the Chicago stockyards, and this new study by Robert A. Slayton shows that this interest has not dwindled. Based on observation (the author lived in Back of the Yards for three years), over one hundred interviews with community members, and an array of quantitative, descriptive, and photographic evidence, his book adds insights to the knowledge about this neighborhood that has accumulated since the publication in 1906 of Upton Sinclair's *The Jungle* and an ensuing series of studies by successive generations of sociologists from the University of Chicago. Minute descriptions of everyday life in the neighborhood and lively commentaries by interviewees give a sense of authenticity to the reconstruction of various aspects of the community's existence in chapters that evoke an image of belonging and stability.

Particularly engaging are the chapters on the pursuits of children and adolescents (chap. 2), the activities of women outside the home in the parish and in the neighborhood (chap. 3), and the religious and church life of this largely Slavic and Catholic community (chap. 5). These chapters probe areas insufficiently investigated thus far by urban ethnic historians. Also interesting, if controversial, is the chapter on the development of national ethnic associations and their alienating and undemocratic character as opposed to that of the local immigrant societies (chap. 6). The part (chap. 9) on the creation of the Back of the Yards Neighborhood Council in 1939, the oldest community group in the United States still in existence, "which managed to overcome nationalist barriers and achieve community-wide goals" (p. 13), is informative and written with the passion of a committed social worker. Surprisingly, however, although Slayton discusses the uneasy rapport among different (white) ethnic groups in the area and the resulting prolonged fragmentation of the community, he remains silent about the Back of the Yard residents' relations with blacks. (The notorious race riots of 1919 in which the neighborhood was involved are not even mentioned.)

The book's central idea is that the working people of the Back of the Yards created "small worlds of

local democracy" in their families, immigrant associations, and parishes. Drawing attention to these democratic impulses in the local ethnic working-class society, which have gone largely unnoticed in the existing literature, is an important contribution of the study. Therein, however, also lies its major weakness. First, Slayton's definition of this grass-roots "democratism" remains vague throughout the analysis and shifts from local control by the people over their lives and inclusiveness of participation involving all groups and segments of the community to the egalitarian structure of authority and decision making. Second, and even more important, the argument about the inherently democratic nature of the local institutions is overdrawn, and the thesis is asserted through frequent repetition rather than demonstrated by concrete evidence. Slavic peasant-immigrant culture and East European Catholicism contain both democratic and authoritarian, nonlibertarian elements, and considerable ambiguity and tension have always existed between the two. The author himself repeatedly, albeit only in passing, notes the undemocratic tendencies in the local settings he describes: the authoritarian rigor of the ethnic parochial schools and parental support for it (pp. 49–51), the pronounced gender inequality in the immigrant homes in which men dominated women (pp. 74–75), the rigid, undemocratic ways of the local priests (pp. 79–81, 122–25, 175–77), the intolerant social control of the local community that castigated "deviants" (p. 117), and the undemocratic practices of local ethnic societies and the authoritarianism of their leaders (pp. 163–71). Slayton's study would have only gained in wisdom and persuasion had he considered this evidence against his major contention and produced a less forceful but more accurate, complex interpretation.

I must add a final technical comment. Sadly, but unavoidably, the knowledge of "ethnic" languages has almost disappeared among the younger generations of Americans. Still, students hoping to reconstruct faithfully the lives of their subjects should pay careful attention to the accuracy of their linguistic renditions. Although generally satisfactory in this regard, the book contains some annoying errors, which easily could have been removed by native-speaking editors. For instance, in Polish, minor spelling errors are made such as "popraveny" instead of *poprawiny* (p. 66) and "carta" for *karta* (p. 78), as well as mistakes such as "kuchina" for a goose-down comforter (p. 71) and mistranslations that make inaccurate the author's accompanying comments, such as *okolica*—a social-geographic area rendered as "reputation" (p. 113).

EWA MORAWSKA  
University of Pennsylvania

EDWARD ABRAHAMS. *The Lyrical Left: Randolph Bourne, Alfred Stieglitz, and the Origins of Cultural Radicalism in America*. Charlottesville: University Press of Virginia. 1986. Pp. xiii, 265. \$20.00.

This book is neither a history of the Lyrical Left nor a systematic study of the origins of cultural radicalism in America. It consists, rather, of two long biographical essays—one on Randolph Bourne and the other on Alfred Stieglitz—sandwiched between introductory and concluding chapters that attempt to generalize the importance of their lives and beliefs. The heart of the argument is that, different as these two men may seem, each made essential contributions to "the movement" of cultural radicalism in the years before World War I. By examining the development of the ideas of political-cultural critic Bourne and champion of modern art Stieglitz, Edward Abrahams expects to offer "insights into the nature and significance" of the subcultural generation that included John Reed, Max Eastman, Floyd Dell, and the rest of "the usual suspects" who cavorted in Greenwich Village during its heyday (p. x).

The effort to say something original about these young men and women who believed they could change the world by writing short stories, putting paint on canvas, founding publications, and living with members of the opposite sex without benefit of clergy proves to be impossible. Indeed, the beliefs outlined here have been detailed in half a dozen previous works of history and biography. We already know too well that such radicals rejected "bourgeois . . . materialistic life" as "personally stifling and socially reactionary," detested the ideas of social control proposed by Progressives and *New Republic* intellectuals, and wished to "use art to liberate themselves and society from what they considered to be artificial abridgements to personal and social freedom" (pp. 2–3).

To such familiar notions, the work does, happily, have some things to add. As he describes the careers of these two individuals whose backgrounds are so different (Bourne was from a poor family and Stieglitz was bred to wealth and culture) and who lived intellectually in very different realms (Stieglitz knew little of politics and Bourne nothing of visual arts), the author gives a sharp sense of the diverse elements contained in the Lyrical Left. And, as he details the growth of their ideas (Bourne was a generational thinker, a prophet of feminism, cultural relativism, and the notion of revolution based on youth, whereas Stieglitz saw artistic and spiritual vision as the locus of social change), he brings alive again the hopes of such radicals in the happy period before they were dashed by the war and the Russian revolution.



In truth, Abrahams's willingness to take the ideas of Bourne, Stieglitz, and their generation seriously and to place them in the context of their times is one of the refreshing contributions of this work. In recent years a good many scholars (including Christopher Lasch, Robert Humphrey, and Leslie Fishbein) have enjoyed bashing cultural radicals for ideological errors and intellectual shortcomings but have entirely ignored the importance of the powerful feelings that moved them to action (and can still move us). This book works to redress the balance by portraying two cultural radicals as whole, if occasionally naive, human beings. And, although the author dreadfully overuses the terms "alienation" and "modernist" (and ducks the issue of whether his subjects ever used them), he is still to be commended for restoring Bourne and Stieglitz to us, complete with warts and dreams.

ROBERT A. ROSENSTONE  
California Institute of Technology

IAN TYRRELL. *The Absent Marx: Class Analysis and Liberal History in Twentieth-Century America*. (Contributions in American History, number 115.) New York: Greenwood, 1986. Pp. xii, 270. \$37.50.

Tracing the history of a void is difficult at best. Yet this is what Ian Tyrrell sets out to accomplish. He argues that American historians have omitted Marxist concepts because of a deeply embedded methodological bias and develops his position by tracing historians through the traditional periods of American historiography.

Tyrrell maintains that since the period of professionalization American historians have accepted the methods of late nineteenth-century positivist, empirical, and antitheoretical philosophy. These historians postulated an "observer-fact distinction," which remained the customary starting point for most who followed. The New Historians and the Progressives, who arrived at different interpretations, did not question the prevailing scientific assumptions. Although *An Economic Interpretation of the Constitution*, for example, illustrated how economics impinged on politics, even Charles Beard acceded to empiricist methods by reiterating the maxim, "No documents, no history." Scientific historians controlled departments, colleges, state universities, and scholarly production. They were thus able to absorb new progressive interpretations but retain old methods.

The "consensus" school extended the liberal, present-minded interpretations of American history begun by Progressives but did not reconsider positivist-empiricist methods. Although consensus historians adopted new concepts from the social sciences, they were pressed politically and socially by anticom-

munist hysteria, threatened by academic repression, and faced with government funding of business histories. So threatened, they responded by defending American democracy and pluralism against "totalitarians."

Even the New Left of the 1960s failed to overcome the liberal and positivist legacy. Young radical historians either left the academic world for full-time activism or simply continued under the delusion that more facts would result in better history. Not the New Left but the new social history linked to it first opened doors to Marxism in the 1960s. Studies of oppressed workers, slaves, and women have focused attention on class, social structure, and ideology.

Now—through the leadership of Eugene Genovese, David Montgomery, and Herbert Gutman—a group has begun to question empiricism and make use of recent social and historical theories from Britain and France. The Annales school, especially the work of Fernand Braudel, and the suggestive syntheses of E. P. Thompson, Immanuel Wallerstein, and Barrington Moore, Jr., present integrated studies rich in possibilities for application to America. Especially useful is the focus on slow shifts in cross-national structures, which could link more superficial political and social history.

Tyrrell has command of an imposing array of historiographical materials. He also attempts to trace the "reproduction of knowledge" through several generations by noting internal professional divisions, changes in academic funding, trends in the job market, and so on. But such a complex study demands a structural clarity that is not present. And Tyrrell is inconsistent, arguing that the omission of Marxist theory stems from both method and ideological preconceptions.

Also, Tyrrell implies a two-part periodization that contrasts what might be called the systems-Marxist phase (after 1960) with the positivist-empiricist phase (before 1960). But such duality distorts the picture. Liberal history took its impetus from many sources, including Marx. Richard Hofstadter, for one, was deeply and emotionally affected by the politics and philosophy of Marxism in the 1930s and incorporated some of its tenets into his thinking. Further, Tyrrell does not assess the impact of political sources of Marxist thought such as the Socialist Labor, Socialist, or Communist parties. Instead, he overuses secondary accounts and presents little that is new for the period before 1960.

Tyrrell's treatment of the period after 1960, however, is compelling. Here he is reflective and insightful. His criticisms of such people as Eric Foner, Barbara Epstein, and David Montgomery and his

suggestions for revising old Marxist concepts deserve careful consideration.

SUSAN STOUT BAKER  
Southwest Missouri State University

ROBERT MOATS MILLER. *Harry Emerson Fosdick: Preacher, Pastor, Prophet*. New York: Oxford University Press. 1985. Pp. xvi, 608. \$34.50.

This volume expresses the author's admiration of an admirable man. In many parts of the book Robert Moats Miller succeeds in his aim to make Fosdick's life accessible without hagiography. In others, however, he does not. But, hagiography or not, this book represents ten years of research in a vast store of manuscript and printed material and provides a sober historian's check on many blind spots in Fosdick's justly famous autobiography.

Miller intends to set the man fully in the context of his times. The seventh of his thirty chapters, on the fundamentalist-modernist controversy that drove Fosdick from his Presbyterian pulpit in New York to the pastorate of the new Riverside Church, is a splendid addition to historians' knowledge of that troubled era. The two chapters on liberal pacifism between the world wars yield a balanced understanding of that movement. Yet Miller fails to see how much the peace crusade was undergirded by the evangelical Christian pacifism that was widespread at that time or how much the liberal "progressives" retarded the evangelical movement by claiming a monopoly on the advocacy of peace. The chapter on Fosdick's trip to Palestine between his two New York pastorates will restore memories of the respectability of anti-Zionism and of the humane and pacifist "spiritual" Zionism of Judah L. Magnes, founder and long-time chancellor of the Hebrew University of Jerusalem. Fosdick's essential Victorianism, tempered only by his championing of the educated woman's favorite issue, birth control, comes through forcefully in both his commitment to temperance and his opposition to modern art. Miller is most critical of the famous pastor when describing the tendencies to inaccuracy and to sentimentalism in his sermons and their accessibility to his wealthy hearers. They circulated across the country because of Fosdick's long tenure on the widely broadcast "National Vespers" hour.

Other chapters border on hagiography. In his chapters on the evolving social gospel and on pacifism, Miller manages to make Fosdick look good at the expense of Reinhold Niebuhr, whose realism has recently fallen from grace. Those dealing with the character of the Riverside congregation frequently note the wealth and social status of its prominent members. John D. Rockefeller, Jr., appears here scores of times. But Miller never exam-

ines the effect of Fosdick's loyalty to them on his neglect of the rigorous calls of both the Hebrew and the Christian Scriptures to forsake greed, disdain riches, and care for the poor. The chapter on racial justice uses the grounds that Fosdick himself gave to excuse his failure to recruit black members from nearby Harlem, namely, his antipathy to "stealing sheep." That antipathy did not seem to apply to well-off whites, whom he recruited with abandon.

Miller made the "dubious decision not to barnacle this narrative with citation footnotes" (p. x). For a scholar of his stature, these might have been dispensable had he more often provided the time and setting of quotations from Fosdick's printed and unprinted materials that he used to counter persistent criticisms. This is particularly difficult because, as Miller also tells us, Fosdick throughout his life repeatedly took time to weed out from his papers and correspondence what he regarded as unimportant material. Would some of that material have altered the future biographer's judgments? Would other extant material have done so?

Here, then, is a volume that does for Fosdick and his wide circle of liberal New Yorkers something akin to what Allen Nevins did for John D. Rockefeller, Sr. The book merits the same examination. It will pass much of it.

TIMOTHY L. SMITH  
Johns Hopkins University

JOHN T. KNEEBONE. *Southern Liberal Journalists and the Issue of Race, 1920-1944*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1985. Pp. xx, 312. \$26.00.

As the South rebounded in the late nineteenth century from the effects of the Civil War and Reconstruction, a small group of influential southerners called for creation of a "New South" founded on democracy, industrial progress, and racial stability. The reversion of power to the planter elite and the establishment of the Jim Crow system hindered their efforts, but they nonetheless precipitated a social and political debate that prepared the South for modernization and cultural assimilation. John T. Kneebone's study provides a rich intellectual history of a new generation of southern liberals caught chronologically between their New South progenitors and the advocates of civil rights militancy in the 1950s, 1960s, and 1970s. Relying for the most part on published writings—books, articles, and editorials—Kneebone focuses on five people: Gerald W. Johnson of the *Baltimore Evening Sun*, George Fort Milton of the *Chattanooga News*, Virginius Dabney of the *Richmond Times*, Hodding Carter of the *Greenville Delta Democrat-Times*, and Ralph McGill of the *Atlanta Constitution*. Together, according to Kneebone,

they constituted the heart of southern liberalism, trying to retain a regional identity while assisting the South in adjusting to the social values of modern America.

Kneebone defines southern liberalism as a small but influential cultural movement based on a strong commitment to industrial progress; opposition to the moralistic crusading of the Ku Klux Klan, religious fundamentalists, and prohibitionists; hope for liberating southern politics from the parochial oppression of racial bigotry and rural agriculturalism; and a desire to bring about a "vertical segregation" of the races. The race issue, of course, eventually trapped southern liberals in their own rhetoric. Disgusted by racial violence and anxious to improve the lot of southern blacks, they nevertheless believed change would have to come within the Jim Crow system. Like Booker T. Washington, they suspected that social and political agitation would only bring down the wrath of white bigots. But what Washington could say as a black man in the early 1900s was impossible for white men in the 1930s, 1940s, and 1950s. "Vertical integration" actually meant "separate but equal," and, although their desire to improve black schools, employment, housing, and access to public facilities was well intended, it collided with the militant desires of black activists for complete equality. Time simply raced past them. They considered themselves liberals but ultimately supported states' rights, local rule, and Jim Crow and opposed federal antilynching legislation, desegregation of the armed forces, fair employment practices and civil rights legislation, and Supreme Court racial activism. Kneebone's book is an outstanding description of their impossible dream, another episode in a series of southern "lost causes."

JAMES S. OLSON

Sam Houston State University

JOHN V. BAIAMONTE, JR. *Spirit of Vengeance: Nativism and Louisiana Justice, 1921-1924*. Baton Rouge: Louisiana State University Press. 1986. Pp. xv, 257. \$25.00.

This is a narrative account of the murder trial and execution of Joseph Rini and five other Italian-Americans in Tangipahoa Parish, Louisiana, in the early 1920s. Describing in detail the events surrounding the case, John V. Baiamonte, Jr., documents the existing hostility toward Italian-Americans, exaggerated fears about the influence of ethnic criminal syndicates, and frequent use of coercion by law enforcement officials. He relies heavily on trial transcripts and extensive newspaper coverage.

The strong antagonism toward Italian immigrants in Louisiana had been dramatically displayed

in New Orleans in 1891 when a large mob lynched eleven Italians accused of murdering the local police chief and thus touched off an international incident. Thirty years later, the Rini case revived these ethnic tensions. In May 1921 the murder of an Independence, Louisiana, restaurant owner by six Italians from New Orleans who were attempting to rob a bank next door outraged local Anglo-Saxon Americans. Despite widespread fears of violence, the presiding judge refused to relocate the trial. The defendants were eventually convicted on the basis of extensive circumstantial evidence. Although prosecutors could not establish which one had actually fired the fatal shot, all six were held equally culpable under state law for the crime.

After the convictions were overturned on a technicality, the six were retried, convicted, and again sentenced to death. Governor John M. Parker, a reformer who, ironically, had been one of the leaders of the New Orleans mob in 1891, declined to commute their sentences. Parker was convinced, despite a paucity of evidence, that the prisoners were representatives of the New Orleans Mafia, whose power had to be destroyed. Even after the trial judge was revealed to be a member of the Ku Klux Klan and one of the six men finally confessed to the murder, Parker adamantly refused to change his mind. In May 1924 all six were hanged in the largest mass execution in Louisiana history.

Despite the inherent drama of the case, the book is poorly written in places and ultimately disappointing. Baiamonte provides excessive detail and insufficient analysis. He fails to relate the incident to other outbreaks of nativism in the 1920s. Yet the study supplies valuable information concerning how the American criminal justice system functioned on the local level in a case involving strong anti-Italian prejudice. It also highlights the problems associated with the application of capital punishment.

CHARLES H. MARTIN  
University of Texas,  
El Paso

GUY ALCHON. *The Invisible Hand of Planning: Capitalism, Social Science, and the State in the 1920s*. Princeton: Princeton University Press. 1985. Pp. x, 252. \$25.00.

Guy Alchon's subject is a system that he defines as "mutually supportive collaboration between social scientists and managerial institutions" designed to establish "America's first peace-time system of macroeconomic management" (p. 3). He argues that the collaboration emerged during the 1920s, building on the prior development of "professional economics, philanthropy, social work, and management as technocratic institutions." World War I

planning experiments, and a brief, abortive effort to use the wartime experiments to resolve "a postwar productivity crisis and a resurgent class conflict." The system included financial support from major foundations, income and business cycle research by the National Bureau of Economic Research, and leadership from Herbert Hoover and the Department of Commerce. The purpose of the system was to serve "as a nonstatist system of countercyclical planning"; the methodology was "to organize and educate individual businessmen to respond to the countercyclical prescriptions of social scientists" (pp. 3-4).

Alchon understands that if we are to know the history of planning in modern society we must learn more about its development within the private sector, especially before 1929. Nonetheless, the scope of his research on the corporations, professional organizations, foundations, and research institutions is narrow. Consequently, Alchon's central conclusions remain little more than hypotheses.

He omits any study of planning within large corporations—for example, their programs for effectively matching supply and demand and their welfare activities—and the extent to which they undertook planning initiatives independently of the Department of Commerce, the foundations, and social scientists.

His definition of "technocratic professionals" (p. 167) excludes lawyers and accountants; he barely mentions engineers and scientists; and he concentrates his discussion of social science on a few economists, particularly Wesley C. Mitchell, and on a few "management scientists," among whom he includes Mary Van Kleeck. He largely ignores those economists, such as Charles Bullock, who had a high degree of confidence in corporate managers and focused their research on business cycles or financial variables.

Alchon's discussion of the foundations is minimal, despite his conclusion that philanthropy's "modern role" is as "financial sponsor, organizer, and protector of technocratic organizations" (p. 52). His evidence pertains almost exclusively to the funding of the National Bureau of Economic Research by the Carnegie and Commonwealth Funds and the support for the *Committee on Recent Economic Changes* by the Laura Spelman Rockefeller Memorial.

He provides little evidence to substantiate either his emphasis on the collaboration of the National Bureau of Economic Research (NBER) and the Committee on Recent Economic Changes with managers or his claim that the NBER institutionalized a "technocratic bargain between social science and managerial institutions" (p. 168).

Alchon makes important substantive contributions in discussing the ideals and aspirations of

certain key individuals. For example, he provides new information on the ways in which Mitchell, Van Kleeck, and Hoover attributed economic instability to business mismanagement. And his evidence that Hoover wished to turn the Committee on Recent Economic Changes "into a force for continuous national planning" (p. 131) is new and especially valuable. Thus, Alchon's book provides a useful exploration of the development of the planning ideas of a few important progressives, as well as a provocative set of hypotheses about the early development of managerial capitalism.

W. ELLIOT BROWNLEE  
*University of California,  
Santa Barbara*

KENNETH S. DAVIS. *FDR: The New York Years, 1928-1933*. Reprint. New York: Random House. 1985. Pp. 512. \$19.95.

As a subject for biography, Franklin D. Roosevelt has now reached the status of a Jefferson; one wonders why yet another volume should be written. And Kenneth S. Davis has himself revealed his doubts about tackling the Roosevelt story ("F.D.R. as a Biographer's Problem," *The Key Reporter* 50 [1984]). His decision to do so grew out of his belief that the vast Roosevelt materials needed to be melded into history. This is the second volume of this history, designed for the general reader but seriously written and well footnoted.

As a contribution to history, it falls short of the goal. It is comprehensive, a very skillful synthesis, but it adds little. Appalled by the volume of the sources at Hyde Park and the National Archives, Davis mostly stayed away from them. New material comes largely from oral histories and from the memories of Eleanor Roosevelt's friend, Marion Dickerman. Intending to provide context and understanding, rather than new information, Davis chose to discipline the subject with a theme and to speculate about relationships and causes. For the most part the speculations are sensible. But his heroic theme—"the struggle to close the power-intelligence gap"—is lost. Hindsight dominates: the governorship and the campaign are everywhere treated as preludes to the New Deal.

The book is reliable on the facts. It might have been a better book if they had been left to speak for themselves. But the author was unable to break through his philosophic idealism to deal with the "pragmatic" Roosevelt in his own terms. Not detecting the man's essential moral and political consistency, Davis assails Roosevelt's lack of logical consistency. He sees Roosevelt's rather "simple" religious faith as the key to his personal strength but does not seem to feel that this had much to do with the

governor's politics or practice. Davis's personal philosophy carries him into strange byways. For example, he portrays Norman Thomas as a logical idealist and makes him the foil for Roosevelt's easy tactical compromises. And he makes Eleanor Roosevelt appear simplistically as a noble lady, whose sufferings set in the worst light her husband's "irresponsible" behavior. Davis also writes much about Oswald Spengler and makes a labored attempt to understand Roosevelt in Spenglerian terms.

But perhaps for all of this Davis's book is well worth reading. Like the first volume, this is very good psychohistory. People come alive on its pages. And the work is impressively thorough on many matters, such as the Brains Trust, the campaign tactics of 1932, and the role of Eleanor Roosevelt. Davis tells the story well, despite some heavy overwriting. But he also makes one think of some important matters that range beyond the immediate tale of a great president's internship in Albany.

ALFRED B. ROLLINS, JR.  
*Old Dominion University*

JAMES C. COBB. *Industrialization and Southern Society, 1877-1984*. (New Perspectives on the South.) Lexington: University Press of Kentucky, 1984. Pp. xii, 185.

JAMES C. COBB and MICHAEL V. NAMORATO, editors. *The New Deal and the South*. (Chancellor's Symposium on Southern History, 1983.) Jackson: University Press of Mississippi, 1984. Pp. x, 173.

These two books analyze the role of industrialization and the New Deal in shaping the contemporary American South. In *Industrialization and Southern Society* James C. Cobb surveys southern industrial development with particular emphasis on the twentieth century. Cobb's thesis is that industrial growth had only a limited impact in modernizing the region. Contrary to the experience of northern states, economic progress did not produce social progress. Instead, southern industrialization reinforced the status quo that insured "labor control, low taxes, and minimal government interference" (p. 1). According to Cobb, the South, because of its dependence on plantation agriculture, could attract only low-wage, labor-intensive industries. An abundance of cheap, nonunion labor has always been the South's major attraction to industry.

Cobb's work is stimulating scholarship, and he has asked important questions, but he has attempted to do too much in a relatively brief study. Many industries receive no treatment, and other topics deserve far more space. The "colonial economy" thesis is worth serious discussion. Interstate and intrastate differences in economic growth should be explained. Why were some states able to forge ahead

economically, while other states remained stagnant? Cobb's thesis, however, can be tested. If he is correct, one can expect to find high positive correlations between economic growth and the social, political, and cultural characteristics associated with economic underdevelopment.

In *The New Deal and the South* Cobb and Michael V. Namorato bring together a collection of essays concerning the impact of the New Deal on southern agriculture, labor, politics, and blacks. The contributors stress the limitations of the New Deal's accomplishments; they discount its short-term effects and make its long-term effects seem largely unintentional. The New Deal's most profound impact was on agriculture. Pete Daniel shows how the Agricultural Adjustment Act triggered the mechanization of agriculture and thus destroyed the old rural structure and prepared the way for large-scale, capital-intensive farming. This result entailed massive human suffering, but Daniel does not say how poor farmers might have been saved. Frank Freidel emphasizes the New Deal's role in sowing the seeds of modernization that grew to maturity in the Sunbelt South, and Wayne Flynt offers qualified praise for New Deal labor policy. The other contributors are more negative. Alan Brinkley argues that the New Deal left the region's traditional political power structure intact. For blacks, Harvard Sitkoff writes, the New Deal was "more potent in promise than in performance" (p. 117). Numan V. Bartley argues that the decade from 1935 to 1945 should replace the Civil War and Reconstruction as the crucial decade in southern history. He clearly believes, however, that World War II created more lasting changes than the New Deal.

Overall, these essays underestimate the New Deal by failing to evaluate the major obstacles that blocked any attempt to alter the South's status quo. They are a reminder that the debate over whether the New Deal was a glass half full or half empty can not produce satisfactory answers. More interest in the interaction between the New Deal and southern institutions is needed.

DONALD HOLLEY  
*University of Arkansas,  
Monticello*

CHESTER M. MORGAN. *Redneck Liberal: Theodore G. Bilbo and the New Deal*. Baton Rouge: Louisiana State University Press, 1985. Pp. 274. \$27.50.

Chester M. Morgan has written a thorough and analytical political biography of Mississippi senator and sometime demagogue Theodore G. Bilbo. Beginning in 1903, when he lost a race for county clerk, and ending with his death in 1947, the work centers on the New Deal years from 1933 to 1939.



Two early chapters give an excellent account of Bilbo's background and early career as well as insight into the intricacies of Mississippi politics. Little is said about Bilbo's personal life or the political and rhetorical excesses of his later years that earned him national notoriety as a racist.

The Bilbo one meets in the pages of this book will be a surprise to those familiar with "The Man" as he was pictured by the news media of the day or by those who have written of him in more recent years. Many will be shocked to see the label of "liberal" attached to a man who said in a senatorial campaign in 1946 that "the way to keep the nigger from the polls is to see him the night before." Morgan bases his estimate of Bilbo's liberalism on a lifelong fight to use political power to help the poverty-ridden white farmers of Mississippi. Morgan shows that as governor of Mississippi from 1916 to 1920 Bilbo achieved the most progressive reforms in the history of the state, including many worthwhile measures in public health, education, and fiscal management. The best claim to liberalism, however, comes from Bilbo's unwavering support in the U.S. Senate for all New Deal domestic programs designed to help the poor. Few senators followed President Franklin Roosevelt and the New Deal as faithfully, almost slavishly, as Bilbo, and few were less rewarded with political patronage and federal projects in their states.

One conventional image of Bilbo is sustained by Morgan. "The Man" was undeniably a political manipulator who used his redneck image and considerable rhetorical ability to berate his opponents and offend almost everyone outside of Mississippi, including the eastern press. Bilbo was so expert at playing the role of southern demagogue that his name has become synonymous with it; Morgan, however, denies that Bilbo used politics cynically or without sincerity. Bilbo, he says, was honestly devoted to the welfare of his redneck followers, just as they genuinely believed in him.

Bilbo is best remembered for his racism, but Morgan establishes that Bilbo, although a racist at heart, did not use race as a political issue until civil rights for blacks became a national concern in the 1940s. But that does not make Bilbo any less of a racist, which Morgan comes close to saying throughout the book.

Morgan lives in Mississippi, and indeed few historians outside of the state would dare, or perhaps care, to write on Bilbo, and fewer still could hope to penetrate the Byzantine depths of Mississippi politics. To say that one must understand the mind of the South or even live there to write such a book would be sophistic, but one must understand the historical context and go to the original sources, which Morgan has done. He has also gone to the pantheon of great Southern historians, and his insight into Bilbo as a Mississippi political phenom-

enon seems clear. This work falls short, however, in analysis of the national reaction to Bilbo and in assessing his importance to the New Deal.

In general, this is a lively and provocative book. Readers will not learn to love Bilbo, but they will better understand "The Man" and along with him a way of thinking and practicing politics that most Americans would like to believe is disappearing from the land.

DAVID E. CONRAD  
Southern Illinois University

JOHN KENNEDY OHL. *Hugh S. Johnson and the New Deal*. DeKalb: Northern Illinois University Press. 1985. Pp. xi, 374. \$27.50.

John Kennedy Ohl's treatment of Hugh Johnson succeeds at two levels: as biography and as an illumination of how "industrial self-government" came to America in 1933 through the National Recovery Administration (NRA). Ohl ably recounts how Johnson achieved overnight celebrity in the spring of 1933 as Franklin Delano Roosevelt's appointee to head the NRA. Colorful, profane, and indefatigable, Johnson promised recovery by fall and sought it by codifying industry and, with much fanfare, through the economy-wide President's Reemployment Agreement (PRA) campaign. During his tenure as the chief of the NRA, Johnson conceded much to the price control provisions business demanded and as little as possible to labor's aspirations under Section 7(a), not merely because this course seemed politic but because he fully shared the business world's conception of "industrial self-government."

The chapters on Johnson at the NRA take up over half the book. By now an extensive literature treats the origins and policy of the NRA, including a number of studies of specific industries. Ohl has mastered this literature and skillfully supplemented it with sources heretofore little used in studies of the NRA, notably Frances Perkins's revealing reminiscences. The result is a balanced and carefully nuanced account of NRA policies toward both labor and business and thereby the most complete and useful general account in print.

The most original segments of the biography are the early and final chapters. The early chapters profit greatly from the availability of fresh materials from the archives and relevant manuscript collections, not least the Johnson papers. Only a fragment of these survive, most of it dating from this period.

These chapters help resolve difficult and enduring interpretive issues that surround Johnson and his relationship to the New Deal and to Roosevelt. Much in Johnson's behavior at the NRA arouses disbelief that he could have retained his credibility

with the public and with Roosevelt as long as he did before being forced out. More fully than ever before, Ohl has recorded with painful detail the excessive flamboyance, bombast, and hyperbole that characterized the PRA crusade and Johnson's style generally—the hectoring and bullying of anyone, inside or outside the NRA, who challenged his sole control of policy or his growing dependence on alcohol. A second issue is Johnson's movement, beginning in 1935, into increasingly vocal and bitter opposition to the New Deal and to Roosevelt, which has made him seem an ingrate, an opportunist, and a minion of conservative businessmen.

From the obscurity of a cavalry lieutenantcy, Johnson rose meteorically during World War I. Ohl argues that he was a sentimental idealist, capable of deep loyalties to people and ideals. His weaknesses were quite apparent—his blustery style with subordinates and associates was evident in his work under Enoch Crowder in the Judge Advocate General's office and on the General Staff during the war, and his alcoholism was a serious drawback in the 1920s during his association with George N. Peek at Moline Plow and later when he became Bernard Baruch's utility man—but his strengths were even more impressive, especially to his superiors. Johnson's ability and usefulness, his colorful personality, and an intangible capacity to develop a filial relationship with older and more powerful men appealed to Crowder and Baruch just as they did to Roosevelt. As for Johnson's opportunism, Ohl's account seems to give absolution. Johnson's association with Peek and Baruch as the army's representative on the War Industries Board marked the turning point in his life and led him to exchange a military for a business career. This initiation into the heady world of the ideology of "industrial self-regulation," reinforced by continuing association with Peek and then Baruch, developed a strong loyalty to a new ideal. After the demise of the NRA in 1935 Johnson became a syndicated columnist and began publicly to criticize the New Deal as it drifted from what he thought were its genuine principles: fiscal conservatism and "industrial self-government." Loyalty to Roosevelt and hope of returning to favor and a government post stayed personal criticism of the president for a time. But in 1937, partly, to be sure, for personal reasons but largely because the New Deal now seemed to be turning irrevocably from Johnson's vision toward bureaucratic statism, he broke entirely with Roosevelt and eventually joined *America First*.

ROBERT F. HIMMELBERG  
*Fordham University*

BETTY GLAD, *Key Pittman: The Tragedy of a Senate Insider*. New York: Columbia University Press, 1986. Pp. xvii, 388. \$45.00.

According to popular stereotype, Key Pittman, who chaired the Senate Foreign Relations Committee from 1933 to 1940, served his nation poorly. Portrayed as a chronic alcoholic more concerned with the price of silver than with international responsibilities, Pittman has gone down in history as the provincial wheeler-dealer par excellence. As his first biographer, Fred L. Israel, wrote in 1963, "His frontier stopped at the Nevada borders," and we have long wondered how a man charged with safeguarding the interests of 130 million people could remain primarily concerned with 110,000 constituents.

Yet, as Betty Glad shows, Pittman's story has long been oversimplified. Glad has written studies of Charles Evans Hughes and Jimmy Carter. This work, based on the Pittman papers, is equally well written and fascinating. The early chapters on Pittman's frontier life could have come out of Rex Beach, and those on the London Economic Conference give credit to a W. C. Fields scenario.

Using psychological theory to good advantage, Glad shows that the tragedy of Pittman lay in the fact that, in some ways, he was a most able legislator, "an effective, intelligent, and shrewd political leader" (p. xvii). Early in his senatorial career he was an articulate and sensible defender of the League of Nations, had some concern for social justice, and was recognized (despite his bias) as an authority on silver and mining. Glad finds that Pittman helped unify the Democratic party in 1924 and 1928, supported the New Deal faithfully despite Franklin D. Roosevelt's lack of gratitude, and showed foresight in warning of Japan's advances. Moreover, he was a hard-headed realist, an able mediator within his party and outside, a skillful tactician in presiding over the Senate, and, usually, a diligent worker. If he permitted foreign affairs to drift, he received little guidance from either Roosevelt or the State Department.

His problems ran deeper than mere alcoholism. Caught in a marriage to a cold and withholding woman, he never received the domestic support and admiration he needed. He fluctuated between feelings of worthlessness and a sense that he was destined for greatness. In a stressful situation, in which genuine initiative was called for, Pittman simply broke down. As long as he served someone such as Woodrow Wilson, whose desires were clear, Pittman functioned as an able political lieutenant. But when he had to cope with the mysterious wishes of Roosevelt, who failed to provide clear and forceful direction, Pittman was truly lost.

The book contains some factual errors. The Federal Reserve Act of 1913 was designed to serve the needs of the business community not to achieve the goals of "Greenbacks, Populists, and other inflationists" (p. 39). Glad confuses journalist Theodore

White with William S. White. Basil O'Connor was Roosevelt's law partner not a "reverend." Burton K. Wheeler was never a "Republican-Progressive." Sidney Bradshaw Fay was not among those revisionists who linked U.S. entrance into World War I with business interests. Peace lobbyist Frederick J. Libby held no doctorate.

Yet, in all, this is a fine biography of an important figure. We will never be able to look at Pittman in quite the same manner again.

JUSTUS D. DOENECKE  
New College,  
University of South Florida

RALPH F. DE BEDTS. *Ambassador Joseph Kennedy, 1938-1940: An Anatomy of Appeasement*. (American University Studies, number 12, Series 9.) New York: Peter Lang. 1985. Pp. x, 263.

As Ralph F. de Bedts notes, British diplomats viewed Joseph P. Kennedy, ambassador to Great Britain from 1938 to 1940, as little more than a rich man, untrained in diplomacy, unlearned in history and politics, and given to publicity seeking to promote himself to become America's first Catholic president in 1940. The British were biased of course, and de Bedts credits Kennedy for his practical intelligence and organizing ability. But, as this monograph—less "an anatomy of appeasement" than a tedious account of a man-on-the-make, who could not distinguish between public and private purpose—shows, Ambassador Kennedy proved to be everything his critics charged.

Kennedy, who "obsessively" sought wealth and status, contributed early to Franklin Roosevelt's first presidential campaign and aspired to be secretary of the treasury. But he had to settle for heading the Securities and Exchange Commission and then the Maritime Commission, until his lobbying, and Roosevelt's overanxiousness to appease Catholic and "isolationist" voters, finally secured for Kennedy the coveted ambassadorship to the Court of St. James—"a helluva long way from East Boston."

Like many others, Kennedy strongly opposed American involvement in European conflict over the Versailles system, feared war's destructiveness and spawning of communism, and insisted that lasting peace required a revamped economic order, including German predominance in Eastern Europe. But, as de Bedts indicates, Kennedy proved a terrible ambassador because he did not seek to foster government policy so much as to present his own ideas—publicly and privately—as official policy and because he did not analyze foreign events but uncritically promoted the views of Neville Chamberlain. Kennedy and Chamberlain found common ground in their overconfident, opinionated styles,

their misperception of the Nazi threat, and their failure to recognize the need to incorporate the Soviet Union or the United States as a countervailing power. Further, Kennedy viewed Spain's Republican government as "a bunch of atheists and Communists," his anti-Semitism made him callous about the plight of European Jews, and he presumed that if he were invited to Berlin he could effect a settlement with Germany—and bolster his presidential prospects. Even after Chamberlain signaled an end to British appeasement in March 1939, Kennedy clung to economic appeasement, insisted that Poland be forced to give way in September 1939, and thereafter proclaimed that the British could not survive the war and that neither they—nor anyone else—should be allowed to inveigle America into the war. Small wonder his hosts deplored him and his president slighted him, although Roosevelt, to be sure, ought never to have appointed Kennedy and lacked the political courage to remove him before the election of 1940.

ARNOLD A. OFFNER  
Boston University

DAVID S. PAINTER. *Oil and the American Century: The Political Economy of U.S. Foreign Oil Policy, 1941-1954*. (Johns Hopkins University Studies in Historical and Political Science, number 104.) Baltimore: Johns Hopkins University Press. 1986. Pp. xii, 302. \$32.50.

This study of U.S. foreign oil policy during World War II and the early cold war is on one level reminiscent of what one might term the "old new left." David S. Painter's "corporatist" interpretation echoes and manifests specific similarities with Gabriel Kolko's argument that wartime oil policy represented the reality of American war aims (*The Politics of War* [1968]). But, in its level of sophistication and depth of research, this book more accurately compares with the post-energy crisis works of Irvine H. Anderson, Jr., Burton I. Kaufman, and Michael Stoff. As do these oil policy specialists, Painter emphasizes the mutuality of interest between business and government and demonstrates sensitivity to the complexity of the issues involved. Accounts of interagency conflict within government illuminate the public sector side, and discussion of the tensions between integrated multinational oil firms and independent domestic producers highlights the danger of viewing the American petroleum industry as a monolith.

The organizational school of recent American business historians has further influenced Painter's approach. His definition of corporatism as "the cooperative use of public and private power in pluralistic, democratic societies" (p. 2) draws from

the work of Ellis Hawley, Louis Galambos, and Gerald Nash, among others. This study therefore also attempts a synthesis between business and diplomatic history as it seeks to explain the reemergence of corporatist strategies to manage the world oil economy.

Painter argues that the "Anglo-American petroleum order," achieved in the interwar period through associative state control of U.S. domestic production and international pacts such as the Red Line and Achnacarry agreements, had broken down on the eve of World War II. In the late 1930s Bolivia's and Mexico's nationalization of foreign oil firms and the opening of vast new Saudi Arabian fields signaled the first cracks in the system. War-time demand and fears of depletion further stimulated U.S. efforts to obtain control of foreign petroleum reserves, but differing perceptions within government often worked at cross-purposes. Whereas the State Department supported a free rein for American companies in a classical corporatist approach recommended by oil adviser Max Thornburg, oil czar Harold Ickes sought a more direct plan of government participation. Painter details the interaction of these differing strategies in the cases of Mexico, Venezuela, Saudi Arabia, and the ill-fated Anglo-American Oil Agreement at the end of the war. Ultimately, the corporatist approach won out.

Chapters focus on the postwar expansion of ARAMCO and its dealings with the State and Justice Departments, attempts to check nationalization in Latin America, the role of Middle Eastern oil in international currency flows, and the Mossadegh affair in Iran. Painter is consistently good when discussing internal political division over tactics and skillful in interweaving important contextual issues such as cold-war tensions between the United States and the Soviet Union, the birth of Israel, and the demise of British power.

Painter has made impressive use of public archival sources in Washington and beyond but has not consulted records of the oil industry. For example, his concise account of the Iranian crisis in the 1950s suffers from the lack of this perspective. We must await the appearance of the next two volumes of R. W. Ferrier's history of the British Petroleum Company to see a version of this story based on business sources. This example illustrates a stumbling block in the path of achieving the corporatist synthesis Painter seeks. Few private firms are willing to open their archives to researchers armed with the thesis that business has largely controlled American foreign policy in the twentieth century.

AUGUST W. GIEBELHAUS  
*Georgia Institute of Technology*

JAMES EDWARD MILLER. *The United States and Italy, 1940–1950: The Politics and Diplomacy of Stabilization*. Chapel Hill: University of North Carolina Press. 1986. Pp. xiv, 356. \$32.50.

This is a thoroughly researched and clearly written study of the successful American attempt to integrate Italy into the Western alliance after its defeat in World War II. James Edward Miller charts the hierarchy of conflicts that characterized policy toward Italy: the friction between the branches of the American government, the tension between the United States and Britain, and the hostility between the West and the Soviet Union. These parts of the story have been told before in more or less detail, but Miller adds a new dimension when he recounts the struggles between the Italian political parties themselves and the work of Italo-American groups to shape the future of their native land.

Miller acknowledges the scholarship of revisionists on the questions he confronts but finally rejects their framework in favor of what I would describe as a form of postrevisionism. Yet a moderate revisionist could agree with much in Miller's understanding. He underscores the view that the central goal of American foreign policy was an orderly middle-class democracy; various economic levers were used to achieve that goal. Nonetheless, in the interplay between means and ends, the ideal of an Open Door political economy gave way to a practical quest for "political stability" in which democracy was preserved but at the expense of the social and economic reform that Miller associates with an Open Door world.

The strength of the book is its rich and dense narrative and not its analysis of ideology and economics. Indeed, Miller's examination raises more problems than it solves for moderate revisionists.

His most sustained exploration is of the period from 1947 to 1949, when the United States funneled money to Italy to destroy the extreme Left in that country. Miller writes that American power assured Italians the right to choose their government and that the United States played a leading role in creating a democratic state (p. 274). But on the same page the author says that American power was employed to force the Italians to choose democracy and that the United States used undemocratic tactics; the United States thus helped undermine the legitimacy of the Italian state and needlessly meddled in the country's internal affairs.

Miller hints that the trouble was an inconsistency in American policy. I think, however, that the issue ought to be the inadequacy of our understanding and not the supposed contradictory nature of the policy. Clearly, to argue that the United States supported democracy when it subverted democratic institutions is illogical. The acceptance of puzzles



such as this by the "postrevisionists" makes their synthesis less than satisfying.

BRUCE KUKLICK  
University of Pennsylvania

KARL DRECHSLER. *Die USA zwischen Antihitlerkoalition und Kaltem Krieg*. Berlin: Akademie. 1986. Pp. 426. 38 M.

This is an East German historian's interpretation of the origins of the cold war. Franklin D. Roosevelt, whose entire political life was bound up with the capitalist class (p. 105), nevertheless realistically cooperated with the Soviet Union to defeat Hitler and plan for the postwar world. Harry S. Truman, inexperienced and uninformed on world affairs, listened to hard-line advisers such as W. Averell Harriman and changed the tone and spirit of American policy, as manifested in the termination of lend-lease to Russia and the American retreat from the Yalta reparations agreement during the Potsdam conference.

Over the next two years Americans moved, (apparently inevitably) away from Roosevelt's cooperation to Truman's confrontation with the Soviet Union. Karl Drechsler documents that process with discussions of various things, including the revolutionary transformations in Eastern Europe, the dispute regarding withdrawal from Iran, George F. Kennan's long telegram from Moscow, Henry A. Wallace's dismissal from the Truman cabinet, General Lucius D. Clay's order to stop reparations shipments from Germany to Russia, Secretary of State James F. Byrnes's signal at Stuttgart "for a new orientation of American policy in Germany" (p. 249), and the crisis of capitalism in the United States.

The turning point in American policy came with the Truman Doctrine, which was the final renunciation of the wartime anti-Hitler coalition and the proclamation of the cold war (p. 289). The Truman Doctrine, the Marshall Plan, and a "new global strategy" made clear that the realistic, rational, objective, and farsighted policies of Roosevelt no longer prevailed in Washington by late 1947 (p. 327). Drechsler documents the "new global strategy" with studies and recommendations of the State Department's Policy Planning Staff, the Joint Chiefs of Staff, the National Security Council, the State-War-Navy Coordinating Committee, and the Central Intelligence Agency.

Drechsler's debt to American revisionist historians of the 1960s and 1970s is noteworthy. The final chapter of the book is an interesting analysis of American cold-war historiography, in which Drechsler says that the New Left historians deserve high praise, even though the pressure of their

surroundings ("Druck ihrer Umwelt") prevented them from completely overcoming their anticommunist and anti-Soviet prejudices (p. 348).

Drechsler says that his sources included materials published in the Soviet Union and the two Germanies (p. 9). His notes and the final chapter reveal how thoroughly he mined American cold-war literature. He also did research in the National Archives, the Roosevelt Library, the Truman Library, and the Manuscript Division of the Library of Congress. There is no evidence that Russian or East German archives were as freely open to him.

JOHN GIMBEL  
Humboldt State University

HOWARD BRICK. *Daniel Bell and the Decline of Intellectual Radicalism: Social Theory and Political Reconciliation in the 1940s*. Madison: University of Wisconsin Press. 1986. Pp. xiii, 280. \$30.00.

If socialism as the goal of a politically dynamic movement in the United States is dead, then Daniel Bell deserves to be designated one of its honorary pallbearers. From his first book, *Marxian Socialism in the United States* (1952), to his erudite explorations of postindustrial society and his claim that the failure of socialism has been the most salient and melancholy political fact of the twentieth century, Bell has probed the implications of the systemic frustration of radicalism, even as he has pondered the recurrent failure of the West to find transcendent meaning and civic cohesion. Such seriousness and pertinence have already inspired Nathan Liebowitz's *Daniel Bell and the Agony of Modern Liberalism* (1985). Now comes a closer and more astute reading that stops its interpretation at midcentury; its pace is so measured that a proportionate intellectual biography would require at least three more volumes.

Howard Brick's intelligence justifies this microanalysis, for he maintains that the determinate structure of the 1940s eased Bell's reconciliation with the status quo. Yet the author makes no sustained effort to show that Bell's thought in the 1940s—which culminated in a short volume in the series "Socialism in American Life"—exhibited intrinsic interest for its acuity. Only in a brief final chapter does Brick relate Bell's conundrums—between means and ends, between bureaucracy and democracy, between rationality and value, between Marx and Max Weber—to his major works of the 1970s, on which his stature as a thinker will most likely depend.

This is not a biography, nor is Bell treated as a member of the species of "New York intellectual." Bell's thought is situated almost entirely in the contexts of the labor issues of the 1940s and, to a lesser extent, the emerging cold war. Bell voted for



Norman Thomas in 1948 while covering the unions as an editor of *Fortune*, and Brick concedes that "significant remnants of radical ideas remained to enrich his thought" (p. xi). But too little is made here of the Menshevik legacy that made Bell's politics so distinctive—a background that no other consequential American intellectual has ever shared. However, although written from an explicitly Marxist perspective, this study is not vulgar: Brick does not blame his protagonist for having reconciled himself to the hegemony of American "imperialism." Bell is treated with consistent respect.

But, like some recent assaults from the Left on the liberal anticommunism of the post-World War II era, the author does not show that Bell overestimated the power of "late capitalism" in the 1940s or where the levers of social and political change might have been found. Indeed, given Brick's own perspective, it is hard to see how Bell could have been expected to remain an effective radical when the confluence of forces was aborting "the historical tendencies that can break the mold of that society to create something new" (p. 211). The conflict between historical determinism and moral agency is an antinomy from which no adherent of Marxism can be expected to wriggle out.

By also stressing Bell's "modernist" sensibility and affinity for a *Zeitgeist* that exalted the sense of complexity and paradox, Brick has presented Bell's work as more enmeshed in complication and contradiction than might have been suspected. The research invested in this book is meticulous. Had the entire corpus of Bell's thought been examined, Brick's sophisticated contribution to intellectual history would have been even more useful.

STEPHEN J. WHITFIELD  
Brandeis University

LAURENCE M. HAUPTMAN. *The Iroquois Struggle for Survival: World War II to Red Power*. (Iroquois.) Syracuse, N.Y.: Syracuse University Press. 1986. Pp. xiii, 328. Cloth \$37.50, paper \$15.95.

Laurence M. Hauptman states that his book is a review of "twentieth century Iroquois history." In a sense, it is more than that. To me, the book is a detailed description of recent events in one part of "Indian country" that typifies the continuing struggle by Indians throughout North America to preserve traditional tribal values and life styles. Beginning roughly around World War II, Hauptman discusses the history of Iroquois settlements in New York, Oklahoma, Wisconsin, and Eastern Canada. The author is most thorough in his attention to the Iroquois in New York.

Recent Iroquois history has many themes in common with that of other Indian tribes in North

America. The pursuit of treaty-based claims to land they once held, efforts to ward off repeated challenges to tribal sovereignty by the U.S. and Canadian governments, adapting to changes in federal Indian policies, campaigns to protect their fishing industries, and endless legal battles to combat the exploitation of their lands and other resources are all a part of the recent history of the Iroquois. Hauptman's book demonstrates that the Indian wars of North America have moved from frontier prairies and woodlands to the courtroom. Tribal lawyers have replaced warrior braves.

Hauptman's analysis of Iroquois history ends with their involvement in the Wounded Knee takeover and the rise of the Red Power movement in the early 1970s. Therefore, very recent developments in Iroquois history, such as the impact of the policy of Indian self-determination as interpreted in the Carter and Reagan administrations, are not covered in this book.

This study is the product of exhaustive research based on an impressive number of historical documents and published materials as well as numerous personal interviews in every Iroquois community in the United States and Canada. The bibliography and endnotes consume over 20 percent of the text. Appropriate photographs are liberally dispersed throughout the book. Unfortunately, the author's detailed research is not used to build a bedrock of support for a lively, easily read history of the Iroquois. Instead, important facts and events are buried in a mass of minor details, some, arguably, interesting but often incidental to the story. Hauptman does not help the reader ingest all of his meticulous research, let alone interpret its meaning. The result is a book that will be of considerable value to someone already interested in Indian affairs, especially the recent history of the Iroquois people. The general reader will be put off by the author's excessive attention to detail.

NICHOLAS C. PEROFF  
University of Missouri,  
Kansas City

STEPHEN E. AMBROSE. *Eisenhower*. Volume 2, *The President*. New York: Simon and Schuster. 1984. Pp. 750. \$24.95.

Stephen E. Ambrose is a first-class historian who has been working on Dwight D. Eisenhower for two decades. This book reflects these facts. Well-researched, richly detailed, and balanced in judgment, this second half of a two-volume biography is sprinkled with sharp insights into Eisenhower the man. It is largely what a good biography should be.

This volume, covering the period from Eisenhower's first election in 1952 through his death in 1969,

is not often self-consciously interpretive; the author's opinions usually are embedded within the narrative. Even in a brief chapter assessing Ike's White House years, Ambrose does not fully engage the numerous scholars who have staked out firm positions on the Eisenhower presidency. The tone is generally admiring. "He is as appealing a human being as President as he was as Supreme Commander," writes Ambrose. "Firm, fair, objective, dignified, he was everything most Americans wanted in a President" (pp. 10–11). In these pages Eisenhower emerges as highly intelligent, confident in his abilities and his judgment of others, sensitive to his public trust, and consistently moderate in his conservative political views. He is portrayed as the clearly dominant force within his administration.

Yet Ambrose does not shrink from rendering critical judgments. He finds Eisenhower to have been insufficiently attentive to the damages inflicted by Joseph McCarthy, insensitive to the moral dimensions of the civil rights issues of the 1950s, and almost viscerally anticommunist in his approach to the developing nations. Building from his book, *Ike's Spies* (1981), Ambrose argues convincingly that Eisenhower greatly increased the role and influence of the Central Intelligence Agency in American foreign policy.

Among the most interesting material is that on the relationship between Eisenhower and Richard Nixon, which Ambrose depicts as highly ambivalent: "there was always a 'but' in the . . . relationship" (p. 593). Also fascinating are the final fifty pages, which treat the post-White House years. Here, Ambrose's writing is at its best, providing illuminating vignettes of Eisenhower's family life, his mentor-like interactions with John F. Kennedy, and Lyndon Johnson's attempts to curry favor with him and use his support to legitimize the war effort in Vietnam. In this later period Ambrose offers his harshest criticism of Eisenhower, for his inept reaction to Barry Goldwater's seizure of the Republican party in 1964.

This study demonstrates both the advantages and the drawbacks of a book unconstrained by the usual limitations on length. Abundant quotations from meetings, conversations, and telephone calls impart a richness of texture that is one of the book's great strengths. On the other hand, the depth of detail on nearly every subject can overwhelm, making it difficult to sort the significant from the insignificant and obscuring the interpretive comments sandwiched within. This problem is accentuated by the relentlessly chronological organization of the book. Ambrose defends his strictly chronological approach as the best way of understanding "the magnitude and multitude of problems that come marching up to the President" (p. 11), but this is debatable.

In a work of such high quality, however, this is surely a minor fault.

GARY W. REICHARD  
University of Maryland

MILTON S. KATZ. *Ban the Bomb: A History of SANE, the Committee for a Sane Nuclear Policy, 1957–1985*. (Contributions in Political Science, number 147.) New York: Greenwood. 1986. Pp. xv, 215. \$35.00.

This valuable work traces the history of the National Committee for a Sane Nuclear Policy (SANE), one of the more important peace groups to emerge in post-World War II America. Milton S. Katz explores the origins of SANE in the mid-1950s and recounts its central role in arousing public awareness of the danger of radioactive fallout through a memorable series of newspaper advertisements, its somewhat craven response to Senator Thomas Dodd's red-baiting in 1960, its troubled passage through the Vietnam years, its near collapse in the mid-1970s, and its recent strong resurgence.

A modest monograph of 171 pages of text, this book admirably achieves its purpose. The comprehensive research includes interviews with Norman Cousins, Linus Pauling, H. Stuart Hughes, Sanford Gottlieb, Benjamin Spock, Seymour Melman, and other SANE notables; materials in the SANE archives at Swarthmore College; and the published works of such scholars as Robert Divine, Charles DeBenedetti, and Lawrence Wittner. The organization is straightforward, and the prose, if not sparkling, is at least clear and serviceable.

The book illuminates the dilemma of middle-class liberals who have sought to pursue their peace concerns while accepting and working within America's political and social system. The frustrations and embarrassments of this position are made abundantly clear. (Benjamin Spock, to his later chagrin, cochaired the "Doctors for Johnson" committee in 1964.) Katz is particularly helpful in tracing and interpreting the controversies that nearly tore SANE apart in the 1960s, as brash young activists, whose radical politics and unbuttoned cultural style were equally unsettling to the staid leaders of the older organization, surged to prominence in the antiwar movement.

Although obviously sympathetic to SANE and its objectives, Katz does not let partisanship blur his scholarly judgment. He offers many perceptive and critical observations about the perils and pitfalls of peace activism in the nuclear age and persuasively demonstrates that SANE's focus on a succession of specific issues—nuclear testing, ABM, MX, and so on—has been both a strength and a liability, enabling it to arouse and mobilize the public's latent but ever-present nuclear fear but also rendering it

vulnerable "to periodic exhaustion of interest, energies, and funds after a particular crisis has been met" (p. 165).

The study ends on an upbeat note, with a chastened but vigorous SANE ensconced in its new Ben Spock Peace Center in Washington, its paid-up membership standing at more than one hundred thousand, and its staff devising imaginative tactics of molding opinion and lobbying politicians. Learning from experience, Katz suggests, SANE now seeks to move beyond *ad hoc* campaigns to a focus on the structural impact of the arms race on the economy and also to move beyond its white, middle-class base through links with labor unions, black organizations, and the Hispanic community. With the latest cycle of peace activism seemingly on the wane, SANE's future remains uncertain. Meanwhile, Katz has provided a balanced, judicious, and illuminating account of its first three decades.

PAUL BOYER  
University of Wisconsin

WILLIAM A. AU. *The Cross, the Flag, and the Bomb: American Catholics Debate War and Peace, 1960-1983*. (Contributions to the Study of Religion, number 12.) Westport, Conn.: Greenwood. 1985. Pp. xviii. 278. \$29.95.

Publication in 1983 of the pastoral letter of the U.S. bishops, "The Challenge of Peace: God's Promise and Our Response," caused widespread comment. Commentary focused primarily on the bishops' surprising challenge to American strategic doctrine, but the larger context, the significant international role of the Catholic church, accounted for the interest the document generated. The church has elsewhere posed questions about the legitimacy of regimes, causing domestic conflicts with serious international consequences. By rejecting the use of nuclear weapons in almost all circumstances, the American pastoral letter suggested that the church might play a similar role in the countries of the North Atlantic Treaty Organization. If the effect of the letter has not been dramatic, at least to date, it may be the first step in a critical development of official church teaching. If it is, then Catholic teaching and the role of the church in the West may become increasingly significant and require more scholarly attention than they have previously received.

In the book under review William A. Au provides a careful assessment of the background of the pastoral letter. He argues that a broad Catholic consensus on just war teaching, combined with the historic patriotism of the church in the United States, allowed the church to provide important moral backing for postwar American defense and foreign pol-

icies. That consensus has unraveled in recent years, producing four distinct schools of thought on issues of war and peace. Realists, led by William V. O'Brien, argue that the fundamental conflict between communism and the Western alliance necessitates revision of traditional ethical criteria to allow the West to maintain that the moral superiority of the West in the cold war is the foundational reality of modern international politics. Pacifists, in contrast, reject this assessment of the central role of the East-West struggle and reject as well the historic nationalism of the church in the United States. Instead, they argue on theological grounds for a Christian pacifism and active nonviolence. Agreeing with the pacifists on the issue of war, the resistance movement associated with Daniel Berrigan and Philip Berrigan goes beyond pacifism to a radical assessment of Western society and culture.

Realism, idealism, and nuclear pacifism embody in varying degrees Max Weber's politics of responsibility: they insist on the need for the church to remain within the prevailing culture and seek to influence its direction. Pacifists and resisters practice a politics of ultimate ends, intensified by a tendency toward the Christian anarchism associated with the Catholic Worker movement. All these groups influenced the dialogue that shaped the pastoral letter of the bishops, which thus reflects the tensions, if not contradictions, of contemporary Catholic thought on war and peace. Au argues that confusion has replaced the clarity offered a generation ago by the realism of John Courtney Murray and the minority pacifism of Dorothy Day and her Catholic Worker associates. Now, realists give due consideration to the political consequences of ethical judgments but ignore the need for personal and ecclesiastical authenticity, whereas pacifists focus on authenticity but do not think through the political consequences of their position. The bishops, Au argues, are impaled on several dilemmas, the most evident being their "strictly conditioned moral acceptance of deterrence" combined with their rejection of almost all use of nuclear weapons. Yet he agrees that they have brought the ethical debate to public attention, and he makes some useful suggestions about how that debate can be moved forward. This is a balanced, informed, and judicious account of an important development in recent American history and a useful introduction to contemporary Catholic thought on war and peace.

DAVID J. O'BRIEN  
College of the Holy Cross

#### CANADA

R. DOUGLAS FRANCIS. *Frank H. Underhill: Intellectual Provocateur*. Toronto: University of Toronto Press. 1986. Pp. x, 219. \$27.50.

Frank Underhill is best remembered in Canada as a historian whose essays and lectures brought refreshing perspectives to the study of political ideas and the liberal tradition, as an intellectual gadfly who contributed iconoclastic pieces to *The Canadian Forum*, cofounded the League for Social Reconstruction (the brains trust of the social democratic Left), and drafted the Regina Manifesto of the Co-operative Commonwealth Federation, and as a defender of academic freedom in 1940–41 when he was threatened with dismissal from the University of Toronto. Although these aspects of his career have been thoroughly investigated by others, R. Douglas Francis has reexamined them within the framework of Underhill's personal intellectual history. He tells us more about Underhill's family, his education at Toronto and Oxford, and his experiences at the University of Saskatchewan in the 1920s, but on important matters such as the evolution of Underhill's political philosophy Francis confirms and adds detail to the story that Underhill himself told in his retrospective papers.

This succinct and perfunctory book is not quite a biography and falls short of being an altogether convincing intellectual portrait. It offers few revealing glimpses into Underhill's personal life and contains almost no probing beneath explicit statements to explain his obsessive negativism—he seemed to ignite as a writer only when he had something to attack or denounce—or why he blamed others for his failure to live up to his early promise and many unfulfilled commitments. Francis seems unable to make up his mind about the possible personal roots of this strain of negativism, writing at one point that Underhill's biting sarcasm was intended to stimulate debate rather than to express strong convictions (p. 61), and, at another, that Underhill believed strongly in what he was saying and was prepared to suffer for his convictions (p. 100).

Underhill's thinking had a derivative quality, and Francis says more than he perhaps intended in writing that his subject found reading the best way to work through a problem or to generate new ideas. Underhill was a channel through which a succession of currents—English Fabianism, American Progressivism, and cold-war liberalism—were conveyed to a segment of the Canadian intellectual community. He seemed, at worst, an echo, at best one who applied liberal thought from abroad to Canadian conditions. This study exaggerates Underhill's originality because it examines him in a restricted context.

Francis, finally, accepts at face value Underhill's view of intellectual leadership (which was modeled after the archaic Victorian ideal of the intellectual as a public moralist and sage), and he seems unwilling to inquire into the relationship between the changing position of the university intellectual in Cana-

dian society and Underhill's self-proclaimed public role.

This is, in short, a commemorative rather than a critical study that leaves much unexplored.

CARL BERGER

*University of Toronto*

GERALD M. SIDER. *Culture and Class in Anthropology and History: A Newfoundland Illustration*. New York: Cambridge University Press, 1986. Pp. xi, 205. \$34.50.

Gerald M. Sider argues that the definition of culture as shared value that integrates and gives meaning to a whole society is too descriptive and too epiphenomenal to participate in any measurable way in the making and remaking of social systems. To remedy the situation he redefines the concept of culture so that it shares this function with class. The core of culture, he argues, is the way in which people perceive and express their mutual relationships. Since culture as shared value does little to assist us in understanding class-based societies, he postulates that a social system may contain more than one culture and one structure and that culture and class meet in the relationship between groups. He tests these hypotheses by examining the relationship between merchants and fishermen in Newfoundland during the period 1830–1960. The nature of that relationship is skillfully drawn. Sider creates a series of vivid and often convincing vignettes by cleverly interweaving selected documents with village customs and aphorisms. He argues that fishermen and merchants are culturally as well as socially different. Moreover, village folk culture was largely a product of the interaction of fishermen and merchants. That culture provided both a bond linking all outport people and a means through which subtle defiance could be offered to the dominant external culture represented by the merchants. The author sees a clear link between the intervention of tradition and the formation of class.

The analysis of Newfoundland raises several problems. Sider's definition of class is not at all clear. The fishermen, apparently, were a class. So, too, were the merchants. So, too, was the state! The definition is complicated by the fishermen's ownership of the means of production and employment of large numbers of workers in the fishery. A similar problem emerges with culture. Sider ignores several obvious mutual relations—religion and gender for example—that may be as important as merchant-fishermen relationships in the formation of folk culture. By not addressing them, he begs the question that they are epiphenomenal. The discussion of denominationalism and political culture is cursory and unconvincing. Finally, historians will be uncon-

fortable with the lack of respect shown for time relationships: customs and events in 1950 are presumed to have had the same meaning that they possessed in 1850.

Sider has remade the concept of culture into a synonym for Thompsonian class. In the process culture is endowed with an analytical function, and the relationship between the merchants and fishermen of Newfoundland is established as an important element in the formation of folk culture. In the process, as well, the mutual relations of other groups are denied any significant role in that culture. This is a valuable work. The author offers a number of insights into social relationships in Newfoundland and raises a provocative and potentially important view of culture.

T. W. ACHESON  
University of New Brunswick

WILLIAM R. MORRISON. *Showing the Flag: The Mounted Police and Canadian Sovereignty in the North, 1894-1925*. Vancouver: University of British Columbia Press. 1985. Pp. xix, 220.

Generally speaking, the older and more integrated a region becomes, the more academic attention it receives; popular and romantic literature remain, but along with them a scholarly appreciation quickens and broadens. Canada's north is no exception. Since the 1960s the change in the research and writing about the region has been dramatic, and the appearance of a steady flow of monographs and articles since then attests to the "professionalization" of Canada's northern history.

William R. Morrison's book is part of this new genre. Extensively documented and carefully crafted, his work has little relation to the Nelson Eddy-Jeanette McDonald approach to the Royal Canadian Mounted Police. Some may lament this because Morrison's study leaves the scarlet tunics faded and their wearers merely human. Tales of hardship and privation—and some heroics—are still there, but the focus is on the role of the police in implementing Ottawa's policy on Canada's northern and arctic frontier. The task was difficult given the vastness of the territory and the primitive state of communications and transportation; lack of any coherent or long-range plan regarding these northern latitudes further complicated their role. Until World War II the north had, in light of more southerly Canadian preoccupations, virtually no development priority. Even after the war the priority appeared the same though more intensive: establishing sovereignty. The police had many functions in the north, but maintaining sovereignty—showing the flag and imposing Canadian law on natives and foreigners (mainly American) alike—in an area

where Canadian claims were tenuous and incomplete was the major reason for their presence.

Most of Morrison's chapters detail the advance of the police, from the early participation in the Klondike gold rush to the establishment of a series of posts on Canada's eastern arctic flank. The most intriguing chapter deals with the police and their early relations with northern natives, especially the difference in their friendly attitude toward the Inuit as opposed to their low esteem for Indians. To the police the Inuit possessed qualities the Indian lacked: thrift, cleanliness, morality, and intelligence. Morrison has also judiciously treated the force's dealings with whalers, politicians, and missionaries, although the Hudson's Bay Company, one of the police's favorite longstanding villains, has not received comparable analysis. But for this omission, Morrison has given us a study that should become the standard in the field.

RICHARD J. DIUBALDO  
Concordia University

ROBERT B. BRYCE. *Maturing in Hard Times: Canada's Department of Finance through the Great Depression*. (Canadian Public Administration Series/Collection administration publique canadienne.) Kingston, Ont.: McGill-Queen's University Press, for the Institute of Public Administration of Canada. 1986. Pp. xii, 278. \$32.50.

After a distinguished career in the Canadian public service Robert B. Bryce decided on his retirement to undertake a history of the Department of Finance. This volume, the first of two he has prepared, provides a brief history of the institution prior to 1930 then examines the major policy issues of the depression decade in detail. The result is a useful compendium of information, even though at times readers may regret that the thematic focus is not stronger.

In looking at the Finance Department's early years, Bryce makes clear that its tasks were mainly bookkeeping and accounting, rather than supplying advice to the government on economic policy. That was primarily a matter of adjusting the protective tariff, which was undertaken by the politicians themselves as they assessed their electoral prospects and the strength of various pressure groups. This situation, in fact, continued through most of the 1930s. In dealing with Canada's international economic relations during the depression, ministers looked to the Department of External Affairs, which was staffed with generalists whose training had been mainly in fields such as history and the classics.

Bryce's arrival at the department in 1938 marked an important transition. He came (by way of Harvard) from Cambridge, where he had been an



early convert to Keynesianism, so that he was in a strong position to spread the new gospel. Not only were other young appointees with training in economics ready to listen but W. C. Clark, the deputy minister since 1932, proved receptive to such ideas. By insisting on holding down expenditures as far as possible in his 1937 budget, the minister of finance had helped end the rather feeble recovery of the Canadian economy and tip it back into recession. That experience strengthened the hands of those calling for a more stimulative fiscal policy and produced the first truly Keynesian-inspired deficit budget in 1939.

Bryce provides a succinct account of the major issues of policy in this period and draws on his own experience where relevant (as when he draws the departmental organizational chart for 1939 to reflect what he considers were the actual rather than the formal relations). Those in search of information on a particular matter will find the book useful, but its organization is somewhat episodic as it moves briskly from one event to another. A little more thematic unity would have been desirable.

CHRISTOPHER ARMSTRONG  
York University

#### LATIN AMERICA

MICHAEL L. CONNIFF. *Black Labor on a White Canal: Panama, 1904–1981*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1985. Pp. xv, 221. \$24.95.

BONHAM C. RICHARDSON. *Panama Money in Barbados, 1900–1920*. Knoxville: University of Tennessee Press. 1985. Pp. xiv, 283. \$24.95.

The Panama Canal became the subject of a substantial literature even before its opening in 1914, but only in recent years has attention been directed toward the history of the black West Indians who provided the bulk of the labor for its construction. Velma Newton's study, *The Silver Men: West Indian Labour Migration to Panama, 1850–1914* (1984), was the first to look in detail at the movement as a whole. The two books reviewed here are evidence of a new phase of research, concerned with smaller parts of that whole. Michael L. Conniff deals with the migrants who stayed on in Panama and their descendants. Bonham C. Richardson, on the other hand, seeks to study the impact of the migration on the island of Barbados.

Conniff demonstrates that the West Indian migrants were set apart by their British orientation and island cultures. At first they were a fluid population, constantly moving back and forth between Panama and the British West Indian colonies. They were

seen by their American employers simply as a third-country labor force and subjected to the social control of police, courts, religion, and education. Native Panamanians showed little antipathy, for the West Indians were confined to the Canal Zone and performed arduous and unattractive tasks. After 1914, however, employment became more competitive, and the Panamanians began to show signs of intolerance. This feeling grew through the depression to peak in the 1940s. During the 1920s a West Indian subculture emerged, which became increasingly distinctive as loyalty to Britain declined and more and more of the community were Spanish speaking and born in Panama. These developments, together with international events, finally stimulated an integration of the West Indian community, and the Canal Zone, into the mainstream of Panamanian culture. Conniff applies "generational analysis" to these phases and provides a lucid, if rather flat, account.

Richardson's work focuses on the impact of the migration on Barbados, the largest supplier of manual laborers. Perhaps forty-five thousand people left Barbados for the canal, from a total population of two hundred thousand. Richardson estimates that about one-half of the migrants never returned to the island. The drain of labor from the plantation economy—most of the migrants were young adult males—was an important factor in the reorganization of the sugar industry that occurred after 1910. Money earned in Panama was equally important in providing down payments for small plots of land and so encouraging the emergence of a peasant class. In analyzing these changes, Richardson tends to try to link every social and economic development with Panama. His attempt to relate the migration to the disturbances of 1937 seems forced, and his observation that Barbadian wages remained virtually unchanged from emancipation to the 1930s suggests that Panama was less than a major watershed. Rather, it was but one of the many factors that transformed the West Indies into commercialized, monetized, highly mobile, twentieth-century societies. If Richardson's interpretation is not always convincing, he does admirably succeed in providing a fresh and vivid picture of Barbados in a little-studied period.

B. W. HIGMAN  
University of the West Indies

ALAN KNIGHT. *The Mexican Revolution*. Volume 1, *Porfirians, Liberals, and Peasants*. Volume 2, *Counter-Revolution and Reconstruction*. (Cambridge Latin American Studies, numbers 54 and 55.) New York: Cambridge University Press. 1986. Pp. xx, 620; xxi, 679. \$49.50 each.

Alan Knight has written an excellent narrative and analysis of the Mexican revolution (1910–20) from “the outside looking in.” He has summarized, synthesized, and expanded on recent scholarship that has examined the history of this turbulent period from the perspective of Mexico’s regions and localities. During the past two decades historians have broken down the Mexican revolution into its component parts and in the process have discovered and rediscovered its complexities. Knight has put the revolution back together. The whole exceeds the sum of its parts.

The book has two premises. Knight argues that the causes, course, and outcomes of the revolution derived from the regions and localities. National issues, ideologies, and leaders were of less consequence than, though not unrelated to, state and municipal disputes and rivalries. This explains, for example, why *cabecillas* (local revolutionary leaders) changed sides frequently with such apparent disregard for ideological principles. National labels—*villista*, *constitucionalista*, and so on—meant little. The author maintains, moreover, that the revolution had a logic of its own, “which cannot be precisely related to the social origins or ideologies of participant groups” (vol. 1, p. 302). Expediency dictated people’s reactions to rapidly changing conditions. The utility of general socioeconomic explanations is, thus, limited (although they are not to be ignored).

Knight presents several controversial hypotheses. He asserts that historians have overestimated the role of economic motivations in the revolution. Rejecting the notion that economic self-interest was a factor in the decisions of either the middle class or dissident members of the elite to oppose Porfirio Díaz between 1908 and 1911, he maintains that political reform was the goal of each. Political discontent was paramount in the fermentation of the revolution. The author also downplays the importance of nationalism in the revolution. Far from being hostile, the revolutionaries, especially those of middle-class origins, saw foreigners as a progressive economic force. Revolutionary leaders used nationalist rhetoric as a political tool; it was not inherent in their ideology. Knight debunks the role of workers as precursors of and participants in the revolution. He proposes that artisans rather than factory workers played a crucial part in fomenting the revolt and that the much ballyhooed involvement of the red battalions on the side of the Constitutionalists in 1914 was superfluous.

The author introduces the concept of *serrano* rebellion. These were revolts of frontier people, from isolated areas, who had a long, fierce tradition of independence and military experience. They sought not to restructure society but to free themselves from the impositions of the central government. These were multiclass movements with no

ideology (least of all for agrarian reform) and without plans or institutional capabilities.

In keeping with his regional and noneconomic perspective, Knight contends that exogenous factors were of limited relevance in the gestation, birth, and course of the revolution and stood “both causally and chronologically at some distance from the revolutionary upheaval itself” (vol. 1, p. 153).

Knight categorizes each successive stage and its leadership from the perspective of the regions and often contradicts notable scholarly interpretations. The Madero regime fell, he argues, not from naiveté but from the inability to deal with unending challenges from the regions. These same challenges defeated strongman Victoriano Huerta as well. The author rejects recent attempts to rehabilitate the drunken general as an ardent nationalist and land reformer. Venustiano Carranza lasted somewhat longer than his predecessors, but he too fell to the inexorable forces of regionalism and the logic of the revolution. The view of Carranza from the outside is particularly effective in refuting the depiction of the First Chief as an agrarian reformer.

The two-volume work presents much more material for discussion and debate. Suffice it for me to indulge myself one such discussion. The author, although he thoroughly explores economic conditions during the era, underestimates the role of economics in the revolution. Knight, for example, discounts the effects of the depression that lasted from 1907 to 1909 (which he labels a recession) on the middle class. To his mind, the middle class did not suffer badly from the depression. But in Chihuahua the number of enterprises run by the middle class declined by 35 percent from 1906 to 1910. The depression abruptly ended nearly a decade of unprecedented opportunity and progress for the middle class. Seventy-five percent of the new businesses begun in Chihuahua during 1905–06 no longer existed in 1909. These setbacks served to highlight the inequities of the Porfirian economic system and translated into revolutionary discontent. Knight also denies that economic self-interest contributed to discontent among groups within the elite. He argues that Francisco I. Madero, the leader of the revolt of 1911, acted directly in contravention of his and his family’s economic interests in opposing the dictator. But the Maderos were in economic trouble in 1908–11. Díaz’s policies regarding the water of the Nazas River and the Compañía Industrial Jabonera de la Laguna jeopardized the Maderos’ holdings. Depression in the mining industry jolted the foundations of their empire. Furthermore, in Sonora *hacendados*, such as José María Maytorena, rebelled in response to the dictator’s ill-considered mass deportation of rebellious Yaquis, which threatened to ruin them by taking away their most valuable laborers. What is most important,

Knight misconceives the relation of politics and business in Porfirian (and revolutionary) Mexico. They were, in fact, inseparable. Local issues concerning taxation, usurpation of land, and arbitrary actions by authorities had as much to do with economics as politics.

This disagreement (and others regarding the importance of external factors, the characterization of *villismo*, and the fermentation process of revolution in the north) notwithstanding, this book is a monumental work of scholarship. It is clearly written, logically and forcibly argued. Knight's use of the reports of the U.S. consuls in Mexico and his grasp of Mexico's regional and local subtleties, variations, and complexities are unprecedented. It is a book to be read again and again to mine its rich interpretation and description.

MARK WASSERMAN  
Rutgers University

DONALD J. MABRY. *The Mexican University and the State: Student Conflicts, 1910–1971*. College Station: Texas A&M University Press. 1982. Pp. xi, 328.

The political activity of Latin American university students has given rise to widely accepted stereotypes. From the Río Grande to Patagonia, according to such views, students are constantly engaged in conspiratorial maneuvering, uniformly (if opportunistically) committed to Marxist beliefs, fervently dedicated to revolution—or at least rebellion—and hopelessly unconcerned about academic achievement. Frequently led by degree-avoiding “professional students,” they represent a monolithic and often formidable force within their national societies.

Not necessarily so, as Donald J. Mabry demonstrates in this study of the relationship between the national university and the state in Mexico from 1910 to 1971—from the beginning of the revolution, through the Tlatelolco massacre of 1968, to its violent aftermath in 1971. Although generally opposed to the ruling establishment, Mabry reveals, Mexican students were almost always divided among themselves. Until the 1930s leading campus activists stood to the Right of the government, not to the Left. Students could also be quiescent, most conspicuously from the 1940s to the mid-1960s, and powerless as well. And when they erupted in overt rebellion, as in the strikes of 1929 and 1968, they were usually reacting to state-sponsored repression, which led in turn to the escalation of small-scale parochial issues into large-scale political conflicts.

Casting such events in historical perspective, Mabry maintains that the Mexican university has become “one of the chief oppositionist centers to the national government” (p. ix). The achievement of

autonomy in 1929 provided the university with the juridical status and intellectual spirit of institutional independence, and “the university became a democratic polity even though the state was not” (p. 57). Tensions thereafter increased, and the symbiotic connection between academia and the government became ever more paradoxical.

The argument is persuasive in its outlines but not in all details. First and foremost, the meaning of “opposition” within the Mexican political system is unclear—especially since, as Mabry himself shows, many student leaders have ended up in the government and not in the hills. The formulation and observation of informal “rules of the game” for student activism also deserve more explicit treatment than they here receive. In what sense was the university “democratic”? And what conditions tend to transform scholastic quarrels into national crises? Does this happen merely because of skull cracking by overzealous police, or does it derive from the overall political environment? Mabry tends to focus on politics within the university and, as a result, he does not offer much speculation on politics around the institution.

In this light Mabry's presentation of the crisis of 1968 is both instructive and disappointing. Quite rightly, he sees it as neither an isolated incident nor a typical town-gown confrontation: it was a transformative event that traumatized the political system and the national society. But to identify Mexico's student activism with the New Left in the United States and Europe is incorrect (pp. 247–48). And it seems insufficient to conclude that, as a result of the killings at the Plaza of Three Cultures, university strikes or movements in Mexico “will forever be different from what they once were” (p. 270). Arguably, the Tlatelolco massacre prompted the alienation of intellectuals from the state, a sociopolitical schism with far-reaching implications in the Mexican context.

But this is a fine book. The research is solid (albeit without interviews), and the writing is clear and unpretentious. Mabry has shown that historical perspective can greatly enrich our understanding of student politics, Mexican politics, and contemporary politics in general.

PETER H. SMITH  
University of California,  
San Diego

JOHN W. F. DULLES. *The São Paulo Law School and the Anti-Vargas Resistance, 1938–1945*. Austin: University of Texas Press. 1986. Pp. ix, 262. \$30.00.

This book concerns a rather small subject, but its publication is justified by the scant historiography on the politics of Getúlio Vargas's Estado Novo

dictatorship (1937–45). John W. F. Dulles, who has contributed extensively to the history of contemporary Brazilian politics, has written a thorough but rather episodic account of opposition to the dictatorship in Brazil's leading institution for grooming national politicians. (Before Vargas came to power in 1930, the São Paulo Law School had educated most of Brazil's presidents.) The study is based on interviews with former student activists as well as ephemeral publications, including rare mimeographed broadsides against the Estado Novo.

Dulles relates the school's politics to national political events, although the two patterns clearly did not always correspond. Yet youthful exuberance and political commitment occasionally interacted, as in the students' exploding stink bombs at a banquet honoring the dictator at the Teatro Municipal in 1938. In 1941 students of the law school organized a strike protesting the University of São Paulo's conferral of an honorary degree on the dictator, and on a number of occasions they issued calls for the return of a democratic regime.

Students at this elite institution were treated better by the secret police than were less well placed opponents of the regime, but some risked their lives, and two were seriously wounded when the police fired on a crowd of protesters in November 1943. Support for the students by leading professors showed the school could pull some strings, because the secretary of security was dismissed from his post

(although he later was made police chief of Rio de Janeiro). The law school's relative ability to affect policies of the regime was enhanced by the existence of a secret society of students and graduates, the *Bucha* (*Burschenschaft*), which had prominently placed members in polity, economy, and society.

Dulles's account makes clear that students formed no solid phalanx against the dictator. Because São Paulo had unsuccessfully revolted against Vargas's government in 1932, students harbored a lingering disposition against him, but no demonstration occurred in the wake of his coup establishing the Estado Novo on November 10, 1937. During the eight-year dictatorship, despite vehement protests from time to time, "apolitical," if not proregime, slates frequently won student elections. After the dictator's fall in 1945, most of the prominent anti-Vargas activists played but minor roles in state and national politics. True, Jânio Quadros became president, but he figures in Dulles's book chiefly as a poet.

In sum, this study clearly shows that the school did not constitute a significant threat to the regime; rather, a number of its students were a persistent source of annoyance and embarrassment to the Estado Novo, however self-sacrificing some individuals might have been.

JOSEPH L. LOVE  
*University of Illinois*

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## Collected Essays

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These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

JAMES CLIFFORD and GEORGE E. MARCUS, editors. *Writing Culture: The Poetics and Politics of Ethnography*. (School of American Research Advanced Seminar, Santa Fe, N.M., 1984.) Berkeley and Los Angeles: University of California Press. 1986. Pp. ix, 305.

JAMES CLIFFORD, Introduction: Partial Truths. MARY LOUISE PRATT, Fieldwork in Common Places. VINCENT CRAPANZANO, Hermes' Dilemma: The Masking of Subversion in Ethnographic Description. RENATO ROSALDO, From the Door of His Den: The Field worker and the Inquisitor. JAMES CLIFFORD, On the Ethnographic Allegory. STEPHEN A. TYLER, The Concept of Cultural Translation in British Social Anthropology. GEORGE E. MARCUS, Contemporary Problems of Ethnography in the Modern World System. MICHAEL M. J. FISCHER, Ethnicity and the Post-Modern Arts of Memory. PAUL RABINOW, Representations Are Social Facts: Modernity and Post-Modernity in Anthropology. GEORGE E. MARCUS, Afterword: Ethnographic Writing and Anthropological Careers.

JEAN JOLIVET, editor. *Abélard en son Temps*. (Actes du colloque international organisé à l'occasion du 9<sup>e</sup> centenaire de la naissance de Pierre Abélard, 1979.) Paris: Belles Lettres. 1981. Pp. 237.

NOËL-YVES TONNERRE, Le comté nantais à la fin du XI<sup>e</sup> siècle. ROBERT-HENRI BAUTIER, Paris au temps d'Abélard. MICHEL REULOS, Sur les pas d'Abélard, du parvis Notre-Dame à l'abbaye de Sainte-Geneviève. MAY VIELLARD-TROÏKOUROFF, L'église Sainte-Geneviève de Paris au début du XII<sup>e</sup> siècle. JEAN-PIERRE WILLESME, Saint-Victor au temps d'Abélard. JACQUES VERGER, Abélard et les milieux sociaux de son temps. JEAN CHÂTILLON, Abélard et les écoles. ÉDOUARD JEAUNEAU, Pierre Abélard à Saint-Denis. JEAN JOLIVET, Non-réalisme et platonisme chez Abélard: Essai d'interprétation. MAURICE DE GANDILLAC, Sur quelques images d'Abélard au temps du roi Louis-Philippe. PASCALE BOURGAIN, Héloïse.

GÜNTER H. LENZ and KURT L. SHELL, editors. *The Crisis of Modernity: Recent Critical Theories of Culture and Society in the United States and West Germany*. (Campus Research, number 486; Publication of the Center for North American Studies and Research, number 6.) Frankfurt am Main, F.R.G.: Campus; distributed by Westview, Boulder, Colo. 1986. Pp. xi, 374. \$27.00.

NORMAN BIRNBAUM, Social Theory in the United States: The Legacy of the Decade 1960-70. DENNIS WRONG, The Concept of Alienation Reconsidered. HELMUT DUBIEL, The Challenge to Progress. RAINER ERD, Why Is There No Corporatism in the United States? MARGIT MAYER, The Political Processing of Urban Grassroots Demands: A Comparative Exploration of Shifts in the Pattern of Citizen Interest Intermediation. MICHAEL DENNING, Cheap Stories: Popular Fiction and Working-Class Culture in Nineteenth-Century America. BERNDT OSTENDORF, The Symbolic Universe of American Forces Television in Germany: "And That's the Way It Is." ALAN TRACHTENBERG, A Note on "Myth and Symbol" as Cultural Criticism. MARK KRUPNIK, Criticism as an Institution. MYRA JEHLEN, The New Radical Criticism and the Limits of Transcendence. GÜNTER H. LENZ, Tradition, Discontinuity, and Counterdiscourse: Some Problems in American Radical Cultural Criticism. ANDREAS HUYSEN, Mapping the Postmodern. SAMUEL WEBER, Demarcations: Deconstruction and Myth: On Gender, Narcissism and Modernity's Discontents. KLAUS POENICKE, "The Invisible Hand": Regression, Deconstruction and the Hazards of an "Ecological" Hermeneutics.

JEROME J. MCGANN, editor. *Historical Studies and Literary Criticism*. Madison: University of Wisconsin Press. 1985. Pp. ix, 298. \$25.00.

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."*

### ERRATA

A letter to the editor by Trowbridge H. Ford in the February 1987 issue, in which he comments on his

review of Albert Venn Dicey: *The Man and His Times* (AHR, 91 [1986]: 922) contains a sentence that Ford wishes to have clarified. The line (p. 267) that begins "While Dicey's mother and father did have a great impact upon his development" should be completed with "it was as a reviewer for *The Reader* that he learned to appreciate J. S. Mill and, as a reviewer for *The Nation*, to appreciate Bagehot and Freeman."

In Michael P. Fitzsimmons's article "Privilege and the Polity in France, 1786–1791" (AHR, 92 [1987]: 269–95), the illustration (p. 288) is incorrectly captioned. Liberty, not Louis, is the figure holding the shield. We regret the error.

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# American Historical Association

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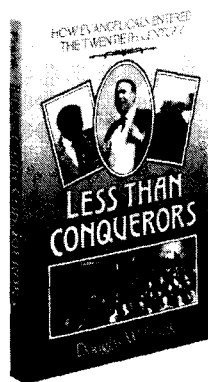
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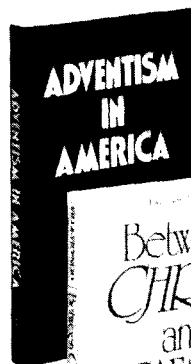
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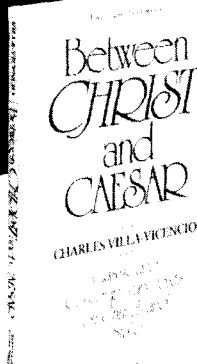
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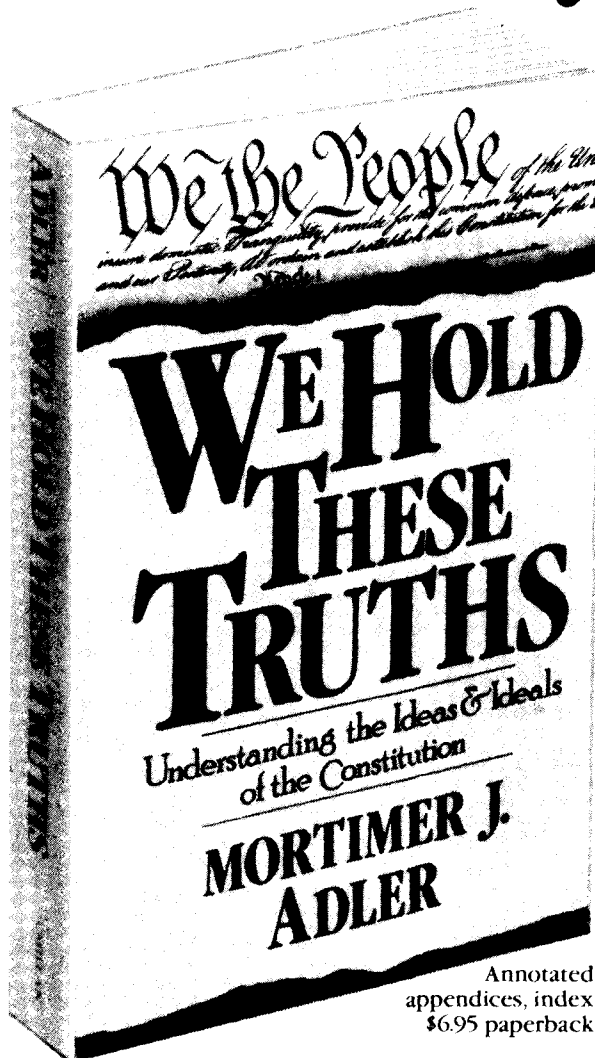


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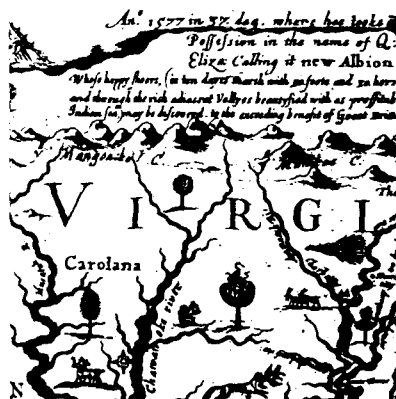
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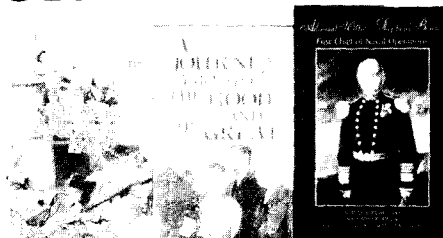
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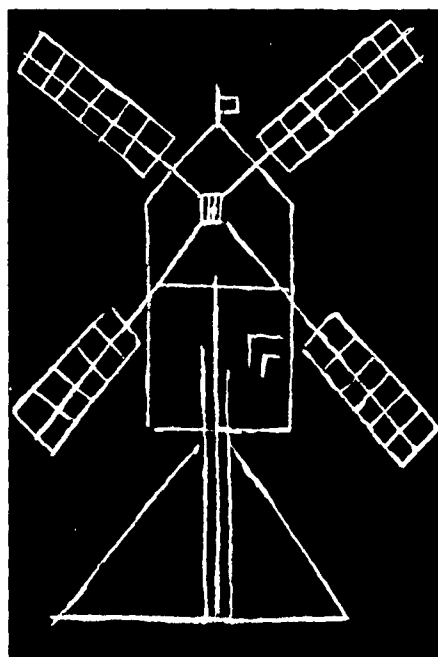
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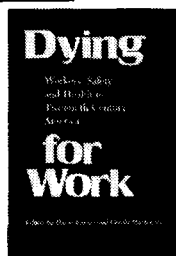
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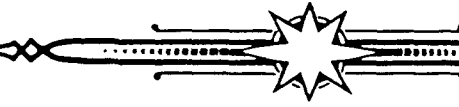
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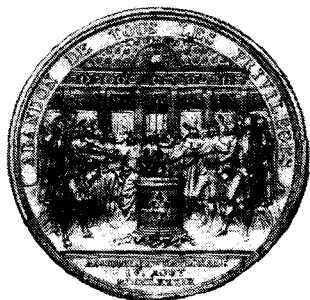
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